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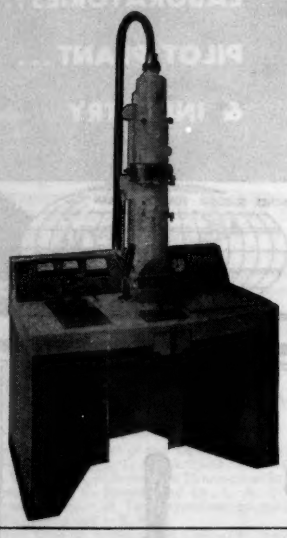

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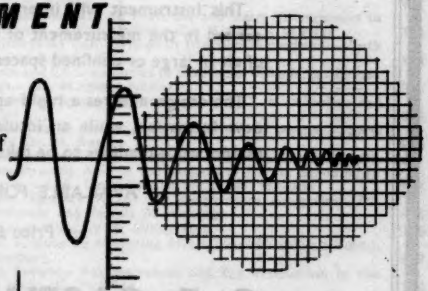

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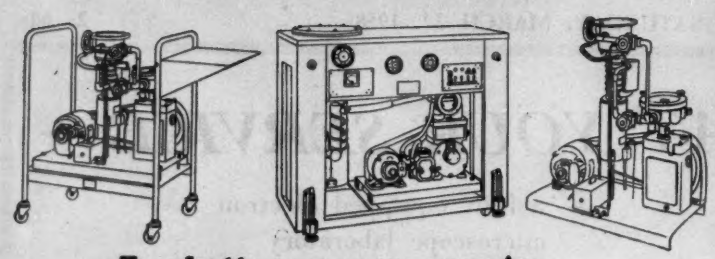

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THE PRESENTATION OF SCIENCE

THE new policy recently announced by the Council of the British Association and discussed in *Nature* of January 4 represents in essence an attempt to improve the communications of science, particularly the understanding of science by the layman, and also the intercommunications of science itself. One aspect of this task is considered by Sir Lawrence Bragg elsewhere in this issue (p. 807). Sir Lawrence is Fullerian professor of chemistry in the Royal Institution, which was founded specifically for interpreting science to the public, although its early history shows that the word 'public' had to be interpreted in a narrower sense than is usual nowadays. However that may be, the Royal Institution has a worthy record in this field, won by the distinction of the professors who have been associated with it; and their distinction has been based on scientific achievement and also on the power to convey their knowledge to others. Both young and older people have been served in this way by the Royal Institution and, as Sir Lawrence comments, special efforts are now being made on behalf of the schools in the London area.

The distinctive feature of the Royal Institution lectures has always been the practical demonstrations, and indeed Sir Lawrence believes that this feature has been, and is of even greater significance than, the substance of the lectures. "The primary way to interest the general public in science is to show experiments and demonstrations and so let them share in the thrill of understanding how things work, which is, after all, a good popular definition of science." The Royal Institution has, of course, special facilities and the necessary technical staff to make such lectures possible, and Sir Lawrence estimates their cost at £5,000 a year. This is not a large sum in these days, and there is much to be said for establishing half a dozen or so such centres, properly equipped for lecture demonstrations, and distributed over Great Britain in accordance with density of population; no doubt some existing buildings could be adapted for the purpose. While this is a long-term scheme, the British Association has already shown how a move in that direction can be made without delay. Working in collaboration with some local education authorities in East Anglia and the Atomic Energy Authority, it arranged a talk on the peaceful uses of atomic energy accompanied by an exhibition of equipment in a school in Ipswich, and school children from the neighbourhood were enabled to attend. This is not the lecture demonstration that Sir Lawrence clearly has in mind, but it is a step forward. It suggests that other national bodies, nationalized industries and some of the larger industrial concerns should consider whether they cannot associate themselves with such a development. They have the technical staff and the equipment, and it should not be difficult, with the collaboration of education authorities and other local bodies, to arrange suitable public lectures.

Having said this much of Great Britain, it is of interest to learn what is thought in the United States about scientific communication—between scientists themselves, and between scientists and the general public.

The sixth annual report of the National Science Foundation had a section entitled "The Communication Report", describing the efforts of the Foundation to improve scientific information services in the United States, including both the information of the public and making federal research results more accessible. A Committee on the Social Aspects of Science, established by the Council of the American Association for the Advancement of Science in 1955, reported in December 1956 on the impact of science on American society likewise, and discussed difficulties in scientific communication and the public interest in science. The latest report of Dr. Caryl P. Haskins, president of the Carnegie Institution of Washington, discusses at some length the fundamental challenge to communication presented by the formidable diversities which characterize the scientific effort to-day.

Dr. Haskins points out that the interlocked character of the present world, the growing similarity of its cultures and the universality of its communication render meaningless any such designation as an Athenian or Elizabethan age; and he suggests instead that it can be characterized in terms of subject-matter, of the loci of ideas, and especially in the natural sciences, where, however, the present age confronts us with communications of almost new dimensions. Diversity of approach, he emphasizes, is the very life-blood of the scientific effort, and under the common rubric of scientists, not only do the observers differ widely in their characteristics, but also their subject-matter diverges even more. Scientific disciplines vary enormously among themselves in their degree of sophistication and in their intricacy; and although the new disciplines must always be in some measure rooted in the older, and the tested approaches of older fields generally have some relevance for the newer ones, the transfer is far from literal. It demands talent and sophistication in the investigator as well as the wisdom and sense of proportion derived from broad experience and a flexible point of view.

These profound diversities among investigators and within the structure of science, while characteristic and immeasurably precious, harbour also all the dangers of fragmentation, and pose the most serious challenges to communication within the core of the scientific effort. Many factors promote the compartmenting of subject-matter which constantly threatens the unity of science. The mere growth of vocabulary and specialization of terminology in a given field may

render its jargon unintelligible even to an investigator working in an adjacent area. Even more formidable barriers to understanding may be presented by words, which are basically the coin of ideas and to some degree their generators, so that some basic concepts, as well as words, which govern scientists in one field of knowledge may be almost, if not quite, unintelligible to those in another.

Differences of language and concept, Dr. Haskins points out, tend to be reinforced by many of the social factors involved in scientific work. The scientist's desire to live and talk with those who will understand his meaning, and the pragmatic influences which lead him to seek identification with others in his immediate field, affect both the investigator and his work. While it means the exposure of each man's work to intimate and continuing criticism by his peers in the same general subject, it can harden an incipient conventionalism and raise the most serious barriers to communication within the body of science itself.

These challenges to communication within the framework of science, which have been taken up by the British Association with its new proposals for its annual meeting and for inter-meeting activities, are the background against which we should consider the further and, to some extent, intractable threat of fragmentation inherent in the very magnitude of the scientific effort itself and the consequent tremendous volume of scientific publication. This threat, as Dr. Haskins notes, has rapidly increased since the Carnegie Institution was established and, while some progress has been made in dealing with the sheer volume of paper, of titles and content, through the use of modern aids for storing and sorting information, intensive research could probably make them much more effective. The Nuffield Foundation is already doing something to encourage this, and a paper by Dr. H. Coblans in the *Unesco Bulletin for Libraries* last July elaborated proposals for new methods and techniques of communication. But the major need is, as was emphasized by Dr. R. E. Fairbairn in a paper read before the Society of Chemical Industry in March 1957, for new thinking about methods of providing information.

Such creative thinking, rather than the improvement and intensive application of mechanical aids, is the only way of meeting the basic challenge of communication inherent in the diversity of the natural sciences. Mechanical aids themselves will not stimulate the fruitful synthesis of ideas even in one field of work, or the transfer of systems of ideas from one field to another from which major innovations of point of view often arise, though their successful application can assist this process by removing obstacles. As Dr. Haskins wisely remarks, the vital factor, apart from the creative thinking already stressed, is a very particular type of environment such as is notably provided by the great universities and the great research institutes. From such environments, comprising communities of investigators, working together in a common mode but in divergent fields, in continuous converse, in sympathy and in rivalry, without predetermined goal, without overcommitment as a body to any given sector or to any one approach to the natural world, have come a goodly proportion of the real conceptual advances which have proved to be landmarks in the history of science.

The real value of the Science Centre in London, to which Sir Robert Robinson gave so much time and thought, during his tenure of the presidency of the Royal Society, lies largely in the contribution which it could render to that end, and it is from this point of view that the folly of successive Governments in rejecting the scheme should be judged. Dr. Haskins's arguments as to the importance of this kind of communication within the sciences at a time when the forces of fragmentation and diversity in science are more powerful and the barriers to interchange higher and more formidable might well stimulate a fresh attack on Government apathy and neglect. It is unlikely indeed that the scheme will be revived until we have in Britain not simply advisers with personal understanding of science, but men of science in posts comparable in standing with the appointment which President Eisenhower has recently made of Scientific Assistant, and also Ministers of State capable themselves of such appreciation.

Dr. Haskins points out that the creation of such an atmosphere is a task to which the Carnegie Institution of Washington is dedicated and for which it is unusually well equipped. As a scientific community, its membership represents almost the full range of gifts and attitudes already described, and its various departments address themselves to scientific fields ranging from the descriptive to the analytical, from the pioneer to the more sophisticated, while the mobility of its organization and its community of spirit preclude isolation either of workers or of fields. Moreover, since its whole work is directed to so-called uncommitted research, it is peculiarly fitted to assist this major task of scientific synthesis. Indeed, Dr. Haskins argues that in the future the Institution's task will be increasingly to lead the way to the breakdown of barriers between the numerous separate fields of knowledge and to the creation of new fields.

The methods used by the Institution, outside its internal substantive work, such as carefully planned and organized symposia and its relation to the universities, are, of course, widely used in Britain by the learned societies and professional institutions, and there are fellowship programmes comparable with the Vannevar Bush Fellowships of the Massachusetts Institute of Technology. As yet, at a time when the isolation of university departments is a matter of common criticism, Britain can, however, offer no such place as the Carnegie Institution in which such senior fellowships could be tenable. In the absence of such an institution, it should be a cardinal object of any strategy of research to provide the conditions which most favour scientific communication and the intermingling of disciplines, either by improvement in the mechanical means of communication or by the establishment of such opportunities as a Science Centre, for example, would provide.

Dr. Haskins, however, suggests that this is not the only challenge to communication to be met, and in

the concluding part of his report he discusses the deeper unities that underlie the diversities of the scientific mode—the unities of value, of standard, of goal and of motivation. The kinds of communication which have had the most profound significance in human affairs have often been, he suggests, neither wholly conscious nor entirely verbal. They have come instead from the sharing of a common experience or a common view, simply and grandly symbolized, and can thus be largely divorced from material stimuli. Few bonds of communication can have been stronger, for example, or have had a deeper influence on the cohesiveness and the world view of a whole people, for good and for ill, than that Augustinian concept of knowledge and research which so dominated early Puritan America. Its exact opposite, however, the approach to Nature typified by the scientific revolution of the sixteenth and seventeenth centuries, has long since proved itself a mode of communication at least equal in its sweep, more flexible and perhaps more viable and, above all, conducive to positive action, and to the growth of ideas and to human joy. This effective entwining of philosophy with practical experiment is now some three centuries old, and Dr. Haskins argues that the very practical success of scientific research and the pragmatic genius of the American people make it imperative to see that these two strands do not become unwound. The inherently more basic element—science as a way of life—requires constant affirmation if it is to retain the vitality, allegiance and comprehension on which its vigour depends, and this is the prime task to which the Carnegie Institution is dedicated. Its most essential duty is to communicate, in virtue of its own mode of being, the essential verities of the scientific way that are too easily forgotten. On one side lie the joy and the underlying human values of the road of the investigator, the compelling life-challenge that is offered to the seeker after ideas about the natural world. On the other lie the great unities of approach and of preparation that bind those dedicated to the scientific path: the requirements of verification; the discipline of parsimony; the emphasis on individual effort.

As Dr. Haskins rightly comments, the scientific mode offers one means by which these priceless elements in human culture can be assured their proper and permanent place. Although it is in relation to the problem of communication that he has emphasized our need to defend and affirm these cultural elements, it might be as well to reflect that to do so may have other important consequences, especially for Britain. Exposition of the cultural values of science might well play a part in drawing into a scientific career a proportion larger than hitherto of the ablest and the most imaginative and constructive minds of the rising generation, upon whom the expansion of scientific and technological education depends; and this, in turn, must be the basis, not only of the material prosperity of Britain, but also of her position in the vanguard of scientific progress.

HUMAN ARTIFICIAL INSEMINATION

Artificial Insemination in the Human

By Dr. A. M. C. M. Schellen. Pp. xii+420. (Amsterdam: Elsevier Publishing Company; London: Cleaver-Hume Press, Ltd., 1957.) 72*s*.

THE practice of artificial insemination has, in recent years, become a commonplace. In cattle-breeding, it is now a well-established procedure, offering many advantages to the farmer. For him, fortunately, it appears to be devoid of controversy. When applied to human beings, as it appears to have been in some form or another since John Hunter, in the eighteenth century, is credited with having advised its successful use by a man with hypospadias, it has aroused strong emotions. Within the past twenty years, human artificial insemination has been used with increasing frequency, and public interest has steadily been growing. In general, insemination with the husband's semen (A.I.H.) has encountered little opposition except by the Roman Catholic Church, which, taking its stand on the fact that it is "contrary to Nature", prohibits it; and by those who will not countenance masturbation, the process by which alone semen can ordinarily be obtained in a form suitable for artificial insemination. Insemination with semen provided by a donor, be he anonymous or otherwise (A.I.D.), is obviously in a different category, since, in addition to any moral or religious problems it may raise, it is also fraught with a wide range of potential legal difficulties, about which the lawyers themselves find no unanimity of opinion. The subject, therefore, is one of far-reaching interest, and it is against such a background that Dr. Schellen's book must be judged.

Several things strike one on reading this book: it is a serious and scholarly attempt to survey the subject in all its minutiæ; it is exhaustive and very fully documented; it deals with many matters which are at most only barely relevant to the subject itself; it is unnecessarily repetitive—a source of irritation to the reader; it contains a great deal of sophistry, much the product of earlier writers to which reference is made, but much the author's own—another source of irritation to the reviewer; but perhaps most irritating of all is the bias which continually intrudes itself, especially against the practice of A.I.D. This would seem to have been introduced without the author's having actually been consciously aware of it, since the final chapter of the book is intended to represent the author's personal views, arrived at as a result of consideration of "all the facets of A.I." and its problems presented "as objectively as we could". The author is a Roman Catholic, and the attitude of his Church has very clearly coloured his emphasis on the numerous objections to A.I.D. and of the less-numerous ones to A.I.H., as well as his interpolations throughout the book. Whereas the literature records numerous highly successful instances of the use of human artificial insemination —not merely from the point of view of overcoming infertility but also from that of the happiness it has caused—Dr. Schellen dwells disproportionately on instances, almost entirely anecdotal, where the outcome has been psychologically or sociologically disastrous.

In spite of these criticisms, and because Dr. Schellen has made such a careful and extensive study of the subject, its literature and, by personal interview with

some of its leading practitioners, its practice, his book is undoubtedly a very important contribution, providing an enormous amount of information on its legal, theological and sociological, as well as purely medical aspects. As such, it commands the attention of everyone whose interest in artificial insemination extends beyond the merely prurient or sensational.

G. I. M. Swyer

SCIENTIFIC PHOTOGRAPHY

Manuel de Photographie Scientifique

(Sciences Physiques et Biologiques.) Par Gérard de Vaucouleurs, Jean Dragesco et Pierre Selme, avec la collaboration de Henriette Faraggi et H. Tellez-Plasencia. Pp. 391. (Paris: Éditions de la *Revue d'Optique*, 1956.) 3,000 francs.

PHOTOGRAPHY has been usefully applied in almost every ramification of scientific work. It is probable that modern photographic techniques could with advantage replace many of the more laborious or less comprehensive methods, in use both in the laboratory and in the field, of obtaining and recording information. This manual makes available a large body of well-classified information (in the old-fashioned sense of the phrase) and is concise and lucid in its presentation. It should be of considerable value to those whose work involves photographic methods.

The earlier chapters are concerned with the properties of emulsions and descriptions of types of apparatus of use in various applications. A photographic darkroom is described and processing and after-treatment of the emulsion are discussed. Then follow sections dealing with special techniques for recording the various spectral bands, electron beams and charged particles. Geometric and photometric measurements are considered in some detail and the remainder of the text is devoted to discussion of several of the more important applications of the photographic method: photomicrography, cinémicrography, photomacrography, document copying, the photography of plants and animals and, finally, of landscapes in the broadest sense. A most useful appendix consists of forty-nine tables of quantitative information and there is a subject index to the whole.

There are some features which might perhaps receive the editors' consideration in another edition. The thirty-eight half-tone plates are not integrated with the text and are often far removed from the appropriate chapters. There is no general bibliography, each chapter having its own, and sometimes (for example, in Chapter 12) authors referred to in the text do not appear in the bibliography. The subject index could well be expanded considerably. Omissions include astrophotography (apart from two plates, of Mars and Finsler's comet respectively), aerial photography and photogrammetry and, more seriously because more generally applicable, stereoscopic photography.

One or two minor improvements suggest themselves: it is possible that estimation of exposures in colour photomicrography simply by measurement of the background (field) brightness might lead to under-exposure with less translucent specimens, and it is doubtful whether such treatment would justify the care in development of the film recommended on p. 202 ($\pm$ 0·25 deg. C. and $\pm$ 10 sec. for first development). Reference might have been made to the ease with which most modern monochrome negative emulsions can be processed for reversal simply and satisfactorily. Perhaps a little more space could have been devoted to the question of depth of field in photomacrography and some mention made of the concept of 'circles of confusion' as an aid to calculation. Typographical errors are few. The page heading on p. 315 is obviously displaced from the following chapter.

This manual will be so useful in laboratories using photographic techniques that early consideration will have to be given to replacing the paper covers by a more substantial binding.

J. Walton Siddorn

YEASTS IN INDUSTRY

Yeasts

By K. Arima, W. J. Nickerson, M. Pyke, H. Schanderl, A. S. Schultz, A. C. Thaysen, and R. S. W. Thorne. Edited by W. Roman. (Biologia et Industria, 1.) Pp. 246. (The Hague: Dr. W. Junk, 1957.) 25 guilders.

THIS book is the first of a series of monographs to be called "Biologia et Industria" designed "to tell the industries using biological raw materials what the biological scientists and also the chemists and physicists can give them, and on the other hand to tell the scientists what industry expects of them". The main sections deal with baker's yeast, brewer's yeast, wine and fruit yeasts, sake yeasts, food and fodder yeast, and yeast preparations.

The articles are very uneven in content. They vary in style from Thorne's comprehensive and concise review of the literature on brewer's yeast with an average of 19 references to the page, to Thaysen's personal account of food yeast production with about one reference to the page. They vary in quality from the lucid and reasonably critical article on the cytology of yeast by Nickerson, in which only three minor errors were noted, to the article on wine and fruit yeasts by Schanderl in which some extraordinary statements are made without proof or reference, and in which errors are frequent. Overlapping occurs between sections as, for example, in the biochemistry of baker's yeast, brewer's yeast, sake yeast and food yeast, each of which is treated separately by a different author. At the same time there are gaps in the coverage so that some aspects of the subject, such as, on the scientific side, the taxonomy and ecology of yeasts and, on the technical side, the behaviour of yeast during distillery, cider and panary fermentations, are almost untouched by any of the authors. For the non-scientific reader the treatment is in general too detailed to appeal, and there are too many errors for the book to be recommended without reserve to the non-critical. For scientific readers the book is neither explicit nor stimulating in its treatment of what industry expects of them; for example, there is little discussion of where new knowledge might be of most value to industry or of where advances in technique are probable or desired. This book, therefore, cannot be said to succeed in the objects quoted at the commencement of this review, objects which are admittedly very difficult to attain.

Although scientific workers may find that in their own particular field the information given is insufficient to satisfy them, yet they should find that in

other less familiar fields some of the articles are of interest and value to them. For example, the articles of Nickerson, Nickerson and Schultz, and Thorne are good and useful summaries of the literature up to about 1952 and in one short section to 1955; Pyke's articles summarize the literature up to about 1950 and give a fair idea of industrial practice; the article by Thaysen gives a very detailed description of the evolution and working of a plant for the production of *Torula utilis*; and Arima's short description of the preparation and composition of sake is probably the only one available in English. The book is adequately bound and printed, it contains eighteen figures and four plates, and carries on its dust-cover the reproduction of a very pleasant painting of sake barrels and bowl.

R. B. Gilliland

MAGNESIUM IN PLANTS AND SOILS

Magnesium

The Fifth Major Plant Nutrient. By Dr. A. Jacob. Translated from the German by Dr. Norman Walker. Pp. xii+159+8 plates. (London: Staples Press, Ltd., 1958.) 40*s*. net.

THE importance of magnesium in crop nutrition has probably been under-estimated in many areas both by growers and to some extent by agronomists also. Dr. Jacob has undertaken through his book to rectify this failing by emphasizing the widespread significance of magnesium deficiency and correctly classifying magnesium as a major element in relation to the effects of nitrogen, phosphorus, potassium and calcium.

The first of twelve chapters deals with the chemistry of magnesium with reference to geochemistry, co-ordination and complex salt formation. The second and longest describes symptoms of magnesium deficiency in more than forty crops. Many descriptions are detailed and effective; some, however, for example, legumes and tomato, seem unduly brief and lacking in detail. Colour plates and black-and-white prints illustrate symptoms in several crops, but a number are either indistinctly reproduced or are not sufficiently close up to reveal diagnostic details. The association of 'sickle-leaf' in cocoa (p. 36) with magnesium rather than zinc deficiency without reference to other evidence may be questioned.

The discussion on ion antagonisms of magnesium is a useful contribution and it is gratifying to find an account of effects of nitrogen supply on magnesium requirements. Four chapters deal with magnesium content of plants and soils, selected methods of determining soil magnesium status, and the balance between plant uptake, soil content and fertilizer practice. In general these subjects are discussed in detail, informatively and widely. A chapter deals with results of fertilizer experiments with general crops including some tropical ones. The account of work on fruit trees and tomatoes is, however, brief and inadequate, though some further details are provided elsewhere in the final chapter on types of magnesium fertilizers.

Animal and human nutritionists will also find useful data in the chapter allocated to this field.

The functions of magnesium in plants are reviewed in Chapter 5, which provides much information on the effects of magnesium on their organic composition. In other respects, however, this important chapter is superficial in its treatment of biochemical aspects of magnesium function. References to specific phosphokinase and transferase enzymes are omitted. The glycolysis cycle and other major pathways of carbohydrate metabolism, many steps of which in plants depend upon magnesium-activated enzymes, are dismissed in some seven lines including but one reference of twenty years ago to 'saccharolytic cozymase', and a few references of doubtful relevance to unspecified phosphatase activity.

It is probably fair to suggest that Dr. Jacob intended his book to be read mainly by agronomists and soil chemists to whom these simplified ideas were addressed. It would be desirable, however, in a future edition to expand this treatment with a more detailed account of the biochemical role of magnesium in plants and in general, for the benefit of readers who do not normally study closely the literature in this field. The inclusion of an index would be helpful.

Soil advisory chemists, agronomists and all interested in mineral nutrition will nevertheless find this book useful and informative and will benefit from an appreciation of the widespread practical importance of providing the optimum magnesium status for soils and crops.

E. J. Hewitt

CALCIUM IN THE ANIMAL ECONOMY

Calcium Metabolism

By Prof. J. T. Irving. (Methuen's Monographs on Biochemical Subjects.) Pp. xi+177. (London: Methuen and Co., Ltd.; New York: John Wiley and Sons, Inc., 1957.) 11*s*. 6*d*. net.

ACCORDING to the final sentence of this book, "there is hardly any physiological action which does not involve the intervention of calcium". A reader unaware of this fact beforehand would certainly agree after reading Prof. Irving's monograph, where are to be found discussions on many different aspects of calcium metabolism. Confronted with so vast a field, the selection of topics for the space allowed must have been difficult. In the event, the choice will please many readers; especially those working on bones and teeth, for whom, in several ways, the author seems to have catered best. Cytologists, protozoologists and physiologists, on the other hand, and others interested in extra-skeletal calcium, may be a little disappointed.

The actual plan of the book seems logical. Dealing first with the calcium value of foods, the question of intestinal absorption is next considered; separate chapters discuss the role taken by phytic acid and vitamin D. There follows an account of the calcium content of the body and its various tissues. This might be made more complete by brief mention of the actual localization of the calcium in the tissues as revealed by methods such as microincineration. Subsequent chapters are concerned with the utilization and retention of calcium, and dietary requirements. There is an account of the calcium of the blood. Next follow three chapters on bone. The first of these deals with the histological features, the formation of bone, and the factors known to influence ossification. The usefulness of this chapter to the

non-specialist reader would be much increased by the incorporation of a few simple diagrams. Next comes a description of the physics and chemistry of bone, followed by a discussion of the role of enzymes in ossification. At this point, the medical reader at least would have been interested to hear modern views on the causation and biochemistry of ectopic calcifications. Finally, there follow chapters on the calcium metabolism of teeth, the excretion and extra-skeletal functions of calcium. In future editions it might be possible to include a brief discussion of the part played by chelate compounds in its mobilization and transport. If, as has been suggested, certain of the cells of bone secrete chelating agents, our ideas about the part played by them will need revision.

This is a most useful review of the subject. The scope is wide, the style is clear and vigorous, and the work as a whole is illuminated by Prof. Irving's wide experience in the field. N. M. Hancox

EPIDEMIOLOGY

Principles of Epidemiology

By Ian Taylor and John Knowelden. Pp. vii+300. (London: J. and A. Churchill, Ltd., 1957.) 30*s.* net.

Uses of Epidemiology

By J. N. Morris. Pp. viii+135. (Edinburgh and London: E. and S. Livingstone, Ltd., 1957.) 17*s.* 6*d.* net.

THIS is the first English text-book of epidemiology, and many first text-books tend to look rather solid and conservative soon after their production. They bring together the important investigations of the immediate past and attempt to confine them within general rules. This book is no exception. Epidemiology, being more a method of investigation than a separate discipline, appears to stand rather shakily on its own feet within these boundaries, and although one can understand its past successes, this book gives no clue as to its future development.

These criticisms, which could be directed at most text-books about an expanding but undefined subject, are not directed at the way this volume has been produced. It is a well-written account of the epidemiology of infectious disease with a large number of examples, and it includes a clear description of the statistical basis of the calculations leading up to the solutions. The assumptions made are those accepted by a graduate in medicine, and to a postgraduate student or to a teaching department this book could be the framework of an adequate and interesting course in epidemiology.

It is a fortunate coincidence that "Uses of Epidemiology" was published at about the same time as "Principles of Epidemiology". Although written for different reasons the two books are ideally complementary. Dr. Morris has felt free to speculate upon the wider applications of the epidemiological methods worked out by the classical school of investigators into infectious disease. The fields considered range from the evaluation of services to specific clinical problems. The flexibility demonstrated appears to be a function both of the possibilities which are present in the use of epidemiological techniques and the author's original outlook towards disease and treatment.

"Uses of Epidemiology" is a short book which can be read pleasantly in an evening. Many of the problems considered are under intensive study at the moment and it is probable that for this reason the book is easy to read now but will very quickly become dated. Some parts appear to be unnecessarily condensed and there is some difficulty in discovering the tables or figures referred to in the text. These disadvantages are minor ones and this series of ideas could be exciting reading to the epidemiologist or any medical graduate, and a gold mine to the post-graduate research worker looking for a subject or a cause. Kenneth Newell

VOLUMETRIC ANALYSIS

Volumetric Analysis

By Prof. I. M. Kolthoff and R. Belcher, with the co-operation of V. A. Stenger and G. Matsuyama. Vol. 3: Titration Methods: Oxidation-Reduction Reactions. Pp. ix+714. (New York: Interscience Publishers, Inc.; London: Interscience Publishers, Ltd., 1957.) 15 dollars.

AS Prof. Kolthoff says in his preface this third and last volume of "Volumetric Analysis" is long overdue. Volumes 1 and 2 of this series were published in 1942 and 1947, and are by Kolthoff and Stenger alone. Volume 3 is a much larger book than either of the preceding volumes and Prof. Kolthoff's account of the necessary changes of authorship reveals the magnitude of the effort needed for its completion.

It treats with commendable thoroughness and detail the practical aspect of one of the largest and most important sections of analytical chemistry. The reputation of the authors will be for most analysts a sufficient assurance of the worth of this book. Its detail is supplemented by a wealth of references, some of them as late as 1956. There are chapters on reactions, indicators and general techniques, permanganate, ceric salts, dichromate, iodometry, Karl Fischer reagent, iodate, periodate, hypohalites, ferrous and titanium salts and miscellaneous titrants. Electrical methods are not generally discussed, but an exception to this is made in dealing with the Karl Fischer method.

The treatment of each reagent includes sections on its preparation, stability and uses. Tests for purity of the reagents and of substances used in their standardization are given. It is interesting to notice that equivalent weights are given in the rational system of Schoorl as well as the international system. Methods of standardization are critically discussed. The collection of carefully selected methods for the determination of various inorganic and organic substances makes the bulk of the book. The scope of the work is evidently confined as in Volume 2 to these useful and reliable procedures, and as in that volume no attempt has been made to cover exhaustively all applications of the methods to specialized subjects in technical analysis. Many such applications are, however, given. Thus Lea's procedure for organic peroxides is included, but details of the bromometric titration of magnesium oxinate as applied to metallurgical analysis are said not to be within the province of the book. A detailed account is given of many applications of the Malaprade reaction.

This book is likely to remain for a considerable time the best and most complete account in the English language of this branch of analytical chemistry.

E. I. Johnson

Birds of Eastern and North-Eastern Africa

By C. W. Mackworth-Praed and Captain C. H. B. Grant. Second edition. (African Handbook of Birds, Series 1, Vol. 1.) Pp. xxv+806+53 plates. (London and New York: Longmans, Green and Co., Ltd., 1957.) 45*s*. net.

FOR those who do not know the first edition of this excellent book the following information may be given: the area covered comprises Sudan, Eritrea, Abyssinia, Somalilands, Kenya Colony, Uganda, Ruanda-Urundi, Tanganyika Territory, part of Nyasaland and part of Portuguese East Africa. 618 species, from ostriches to pittas, are described, species or subspecies being discussed under the following headings: distinguishing characters, general distribution, range in East Africa, habits, nests and eggs, recorded breeding, food, call, and distribution of other races of the species (if any). In the second edition six species and three subspecies have been added as well as a considerable number of notes on distribution, habits, breeding, etc. The small marginal maps showing the geographical distribution of each species described deserve special mention, being not only an excellent idea but also a great and important visual help for the reader. There are fifty-three tables in colour depicting 1,027 species, painted by Roland Green, H. Grönvold, D. M. Reid-Henry, Miss C. E. Talbot Kelly, and N. C. Lighton, and six tables of excellent photographs by Van Someren.

It is a pity that a bibliography has not been included in the book. A work of this kind (even in spite of the authors' intention) is considered by most readers as a reference book, and for this reason its value would be enhanced if it were to contain references to the most important sources of information. A full bibliography of the East African birds is badly needed and the authors are certainly the ornithologists most competent to compile such a work, which might usefully form a third, supplementary volume to this book.

Elsevier's Dictionary of Electronics and Waveguides in Six Languages

English/American, French, Spanish, Italian, Dutch and German. Compiled and arranged on an English Alphabetical base by W. E. Clason. Pp. viii+628. (Amsterdam: Elsevier Publishing Company; London: Cleaver-Hume Press, Ltd., 1957.) 90*s*.

THIS volume is one of an excellent series in which technical terms are defined in English, and then the corresponding words or phrases are given in five other languages. An ingenious arrangement of numbering the terms, which are in alphabetical order, makes it very easy by a simple cross-index to translate a term from any of the other languages. An earlier volume dealt with the terms used in the fields of television, radar and their associated antennæ (see *Nature*, 177, 1099; 1956); the present book covers the corresponding subjects of electronics and waveguides. More than 2,000 terms are defined and they include most of, if not all, the accepted words and phrases used in semiconductor and transistor work, as well as in the general field of electronics. Terms relating to physical electronics are not, however, included since it is intended to deal with the subjects of nuclear science and technology and radiation in a subsequent volume. It is noteworthy that the present dictionary includes definitions already established by the British Standards Institution, the International Electrotechnical Commission and the publishers of the "International Dictionary of Physics and Electronics". As with the former volume referred to above, the book is very well produced in a handy size, with a flexible binding and a convenient thumb-index to the different languages. These qualities combined with the high technical standard throughout make this dictionary a reference work of permanent value to scientists and engineers engaged in the use of electronic devices and microwave equipment.

R. L. SMITH-ROSE

Lecture Notes on the Use of the Microscope

By R. Barer. Second edition. Pp. vii+76. (Oxford: Blackwell Scientific Publications, 1956.) 7*s*. 6*d*. net.

DURING the three years since its first appearance, Dr. R. Barer's little book has become known to a wide circle of scientists; it is evident from the demand for a second edition that this book meets a definite need, by introducing biologists to the correct use of one of their most important tools. As a result of the increasing use of the electron microscope in biology, it might be thought that the usefulness of the light microscope would diminish, but it is evident that the two methods should be used in conjunction, and that the proper use of the light microscope will continue to be of extreme importance. Dr. Barer has deliberately omitted any mention of the specialized forms of modern microscopy, because, as he points out in his preface, these methods are still in a state of flux but the fundamentals of direct microscopy do not change.

There have been only minor alterations in this edition, which resembles the earlier one in retaining its valuable chapters on the theory of the microscope and practical instructions for its use. The appendixes are of particular interest, giving simple experiments to illustrate such points as the effect of numerical aperture of both the condenser and the objective on resolution; the use of Köhler illumination is also described in detail. The book is very well written, and provides a clear explanation of the basic theory of the microscope; although it is intended for biology and medical students, all users of the microscope would be well advised to study it. If they did so, there would perhaps no longer be grounds for the author's opening comment that, "Probably no instrument is more often misused than the microscope".

S. BRADBURY

Old Stone Age

By Stevan Celebonovic, with a Commentary by Geoffrey Grigson. (Nature and Art: a Series.) Pp. 96. (London: Phoenix House, Ltd., 1957.) 30*s*. net.

AS a picture book this volume is delightful and has a real value for the prehistorian. While many of the plates illustrate well-known palæolithic works of art, a number represent sculptures and drawings that are less known and not often reproduced. Frankly, the text is not very helpful, though some readers will find the chronological table at the end useful. Specialists will find little they did not know already, and non-specialists will not learn very much about the art or the cultures that produced it. But, of course, the illustrations are what really matter and at the price they are truly magnificent. No prehistorian should fail to have a copy by him, and the ordinary reader interested in art photography will revel in the beauty of some of the cave and home art subjects as here presented.

M. C. BURKITT

POSSIBLE CORRELATIONS BETWEEN EARTHQUAKES AND MICROSEISMS

By C. A. NANNEY
Department of Physics, University of Chicago

IN August 1954 I observed that a rapid decrease of microseism frequency (inverse period) occurred in coincidence with the Samoa Islands earthquake ($M = 7$–$7\frac{1}{4}$) which occurred at 4h. 42m. G.M.T. on August 18, 1954. The observation was made at the Naval Research Laboratory's Microseism Station at Chesapeake Beach, Maryland. Afterwards, an investigation was undertaken to determine whether the observed change of the microseism frequency might have been correlated with the earthquake, or might have arisen as a chance coincidence, since the microseism frequency is known to vary in time.

To the best of my knowledge, no studies have been carried out to determine whether any correlations exist between earthquakes and microseisms. The following discussion is a summary of some interesting and apparently significant results of such a study.

Microseismic data covering a span of three years were analysed. Most of them (about 90 per cent) were from previously published papers, and the rest were taken by me. The three-year total is the sum of various intervals of data from some eight stations the distribution of which virtually encircles the globe.

Since earthquakes occur fairly frequently, an unbiased sampling method must be used in their selection for a study of this kind. The sampling criteria for earthquakes (such as, all earthquakes occurring during the interval covered by data with magnitude greater than $6\frac{1}{4}$, or all earthquakes with depths reported greater than 30 km.) were established prior to the study of a given set of data. In the subsequent analysis, all earthquakes which fell in the specified category were used, and none was omitted. Similarly, the whole interval of data which were available was always used, and no sets of data analysed were omitted from the statistical tests. The earthquake occurrence times and other relevant information were taken from well-documented sources, such as the "International Seismological Summary". Altogether, some three hundred earthquakes were considered in the analysis.

It should be pointed out that the problem was considered almost entirely on the basis of earthquake information, and no attempt was made to interpret existing meteorological and oceanographical conditions. These conditions are not as well defined as earthquake data (for example, time of occurrence, location, etc.). Furthermore, it is entirely possible that several of the numerous postulated mechanisms for the generation of microseisms may exist at any one time.

Nevertheless, in a statistical study of this type, it is possible to consider the earthquakes independently of any other phenomena which might alter the microseisms. First, if there is no correlation between microseisms and earthquakes, the number of cases contradicting, and the number of cases supporting, the correlation will tend to average out in a statistically large sample of data, and the correlation will not be supported at any statistically significant level. Secondly, any changes of microseisms due to other phenomena will only reduce any correlation that might exist between microseisms and earthquakes, thus requiring a large quantity of data to establish a correlation between the latter, if one should exist.

The findings to be discussed are all based on results which were found to be significant at the 0·01 significance-level. Most of them were significant at a considerably higher level.

The study disclosed that the microseism frequency begins to increase approximately 24 hr. before the occurrence of many large earthquakes. The increase of frequency continued to approximately the occurrence time of the earthquake, and the frequency decreased following the occurrence of the earthquake. The amplitude of the microseisms was found to increase before earthquakes and to decrease afterwards in a similar manner. The average changes, for earthquakes with magnitude $6\frac{1}{4}$ (Gutenberg–Richter scale) or greater, appear to be approximately a 10 per cent increase of the frequency and a 20 per cent increase of the amplitude. The maximum changes that have been observed, which were believed to be associated with seismic phenomena, were an 80 per cent increase of the frequency, and a number of cases of several hundred per cent for the amplitude increase. However, there were some large earthquakes for which no increases of frequency or amplitude were evident.

Microseism data from the following stations were studied: Japan, Guam, Hawaii, California, Missouri, Maryland, New York and Finland. The results indicated that a correlation between earthquakes and microseisms existed at all these stations. The frequency and amplitude effects have been observed for earthquakes which occur at very great distances, even a semi-circumference, from the microseism station. A simultaneous change of these parameters has also been observed to occur for widely separated stations in coincidence with a particular earthquake.

In general, the frequency and amplitude increases occurred only for the larger earthquakes ($M \geq 6\frac{1}{2}$); however, two exceptions were found. First, smaller earthquakes, if closer to the microseism station, were often found to be associated with significant increases in the amplitude and frequency of the microseisms. Secondly, deep earthquakes, though not necessarily large, were found to be correlated with increases of the microseism amplitudes.

The data analysed showed that there is reason for believing that many of the so-called 'microseism storms' may be correlated with earthquakes. This conclusion is based upon a study of a number of papers on microseism storms. From these, the following example is given for illustrative purposes.

Rameriz[1] graphs the microseism amplitudes at St. Louis for about six months of 1938. A study of the microseism storms and earthquakes occurring during this interval disclosed that the time of the maximum

intensity of a majority of these storms coincided closely with deep or large earthquakes. The possibility that the correlation observed might be due to chance was shown to be less than one in ten thousand.

Infrequently occurring, long-period (greater than ten seconds) microseisms are known to exist. A study of microseisms of this type was conducted by Oliver and Ewing[2] during the Atlantic Ocean hurricane seasons of 1954–55. The amplitude of these microseisms often reached a maximum, and rapidly decreased during a time when any existing hurricane, its path and its distance from the microseism station exhibited no unusual change. An examination of earthquake records revealed that a majority of the long-period, microseism-amplitude maxima occurred in close coincidence with either a deep earthquake or a relatively local quake occurring in the Atlantic Ocean. The correlation between a maximum of the amplitudes of the long-period microseisms and the occurrence of one of these two types of earthquakes was very strong and far greater than would be expected by chance. Since the authors[2] point out that a hurricane is not a necessary condition for the occurrence of this type of microseism, there is reason to believe that some of these long-period microseisms may be related to earthquakes, particularly deep ones. Indeed, this is not an entirely new suggestion, as I discovered later. Donn[3], in noting similarities between these and certain types of earthquake waves, has suggested that there might be a relationship between the two.

Numerous attempts have been made to locate meteorological storm centres by means of microseism propagation directions. In many cases these were not completely successful, and were largely abandoned as a consistent means of locating storms. I studied several papers[1,3] where microseism directions were given for times during which no hurricanes were present. This study revealed that the directions found for the propagation of the microseisms sometimes showed rather remarkable agreement with the azimuth directions of the great circle path from the microseisms station to the location of impending earthquakes of significance.

For example, Rameriz[1] determined the azimuth of propagation of microseisms at St. Louis at frequent intervals during the month of November 1938. The majority of these fell in the octant between N. and N.N.E. However, of the six exceptions to this octant, four were found to correspond in time to the intervals during which four significant earthquakes occurred. Two of the earthquakes were probably the largest during the interval for which data were given, the third was relatively close to the station, and the fourth was of deep focus.

However, even more remarkable than this coincidence in time were the results of a comparison made between the four observed microseism directions and the azimuths to the respective earthquakes. The first three cases agreed within three degrees and the fourth case differed by fifteen degrees.

While the directional correlation was greater than expected by chance, I do not believe that the data examined to date are sufficient to constitute a conclusive case for a directional correlation with earthquakes. It is merely pointed out here as being a possibility which has some substantiation, and should be further considered.

The preceding observations offer some hope that they might lend themselves to the prediction of earthquakes. Whether any predictions can be made by utilizing the preceding effects awaits detailed investigations of the earthquake-microseism problem and its ramifications. I hope this article will cause sufficient interest to result perhaps in the genesis of some such investigations.

Even if such an endeavour does not prove successful, the studies should give new insights into the Earth's crust and interior, as well as a better understanding of the little-understood microseisms.

I would like to thank Profs. Harold C. Urey, John C. Jamieson and A. W. Lawson, of the University of Chicago, for the opportunity to discuss this problem with them. I am particularly grateful to Dr. J. E. Dinger, at the United States Naval Research Laboratory, for his continued support and advice.

A detailed report of the study is to be published in the near future.

[1] Rameriz, J. E., *Bull. Seism. Soc. Amer.*, **30**, 35, 135 (1940).
[2] Oliver, J., and Ewing, M., *Bull. Seism. Soc. Amer.*, **47**, 111 (1957).
[3] Donn, W. L., *Trans. Amer. Geophys. Union*, **35**, 821 (1954).

CONTROLLED THERMONUCLEAR REACTIONS

A DISCUSSION on thermonuclear reactions was held by the Royal Society on February 5. The chair was taken by Sir Cyril Hinshelwood, and the discussion was opened by Sir John Cockcroft, director of the Atomic Energy Research Establishment.

Sir John briefly outlined the earlier work which has led to the present successes in the thermonuclear field. He spoke of the discovery of the D–D reaction by Oliphant, Harteck and Rutherford in 1934, of the pinch effect by Northrup in 1907, and of its experimental consequences by Tonks in 1939. Of the more recent work, Sir John emphasized that of Ware in 1948 on toroidal discharges, of Kurchatov on straight pinches, and of Bickerton and Tayler in England, Rosenbluth in the United States and Shafranov in the U.S.S.R. on the use of an axial stabilizing field. Bickerton and Tayler had established that metal walls and axial fields could together definitely stabilize a discharge. After mentioning the work of the laboratories of Associated Electrical Industries, Ltd., and of the Los Alamos group, Sir John briefly described the papers to be presented. He concluded by pointing out that much useful work could be done in the universities in the now unfashionable field of atomic physics on such processes as excitation and ionization in stripped atoms.

Dr. P. C. Thonemann, who read a paper entitled "The Plasma Physics of ZETA", said that ZETA is essentially a large transformer with which high currents are passed through a low-pressure gas. The ring current in the gas forms the secondary of the

transformer. A ring current of this type is self-constricting, but highly unstable: to achieve stability an axial magnetic field is used in conjunction with a metal-wall vacuum vessel. The most significant aspect of the results obtained with ZETA is that stability can be achieved in a toroidal tube.

An isolated plasma is a diamagnetic body, which can remain in an equilibrium position indefinitely in a static magnetic field. As a result of collisions, however, the currents giving rise to plasma diamagnetism decay with time, until finally the gas is no longer confined. The time for which isolation of the gas from the walls can be maintained increases with the electrical conductivity of the gas, which in turn increases rapidly with temperature. On the other hand, high temperatures cannot be produced until isolation from the walls is achieved. Thus the required conditions of isolation and high temperatures cannot be approached stage by stage.

As a result of experiments by R. Carruthers and R. J. Bickerton, ZETA was designed to achieve stability by using metal walls to remove long wave-length instabilities, and axial magnetic fields to remove short wave-length instabilities.

The best indications that the discharge is, in fact, stable are obtained from measurements of the internal magnetic field. Provided the gas can be prevented from breaking up into small pieces, the possible containment-time increases as the square of the linear dimensions of the system. The large size of ZETA has been justified by the containment-times achieved; at present these are only limited by the energy available from the external circuit.

The stable configuration is set up in the following way. Deuterium at a pressure of about 10^{-4} mm. of mercury is weakly ionized by a radio-frequency oscillator. An initial electric field of about one volt per centimetre is applied to the surface of the gas, the ratio of electric field to pressure being chosen to give the maximum rate of ionization without causing the electrons to behave as though they were in an accelerator. As the electron-density increases, current flows predominantly at the discharge surface. When the field due to this current exceeds the axial field initially present, the discharge contracts, and provided the contraction is fast enough the axial flux inside the discharge is conserved. If the metal wall is sufficiently thick no further flux can penetrate it, and the field between the discharge and the walls is just that due to the axial current. Such a system, with sharply separated fields, is known to be stable for short wave-length instabilities. In ZETA, conditions lie between the extremes of completely separated fields and uniformly mixed fields. The time for which such a system remains stable is not yet known.

The metal walls, in addition to excluding the axial magnetic field after the collapse of the discharge, serve to stabilize the discharge against long wave-length instabilities. This is achieved by the image current induced in the wall by any movement of the ring current. The required stability cannot be obtained unless the conductivity of the gas is sufficiently high. This implies almost complete ionization and electron temperatures in excess of 10^5 °K. in the early stage of the discharge, before collapse.

Dr. Thonemann said that the discharge is cooled largely by radiation from impurities, and not by particle bombardment of the walls. Thus, when a probe of area 1/3000th of the wall area is immersed in the discharge, the neutron flux is reduced to an almost undetectable level and the resistance of the discharge is greatly increased. The high temperatures, thermonuclear reactions, and all the other interesting observations follow as a natural consequence once stability has been achieved for relatively long times. If the gas column remains stable as the magnetic fields confining it are increased, then there are no fundamental reasons why temperatures exceeding 10^8 degrees should not be produced.

A paper on "Experiments with ZETA" was read by Mr. R. S. Pease, who said that ZETA is essentially an apparatus for producing high temperatures. The first task, when ZETA began to work on August 12, 1957, was to find the best operating conditions. The guiding principle was that the highest temperatures were likely to be obtained with the largest currents, the lowest starting pressures, and the lowest axial magnetic fields compatible with a discharge which did not damage the apparatus. After considerable experimenting it was found that the best values for the axial field and initial pressure are 160 gauss and $\frac{1}{8}$ μ of mercury (in deuterium). During this experimental period it was noted that alarming voltage transients of up to 1 kilovolt appeared between adjacent liners; this led to concern about the glass-tape insulation between the liners. Another phenomenon encountered at this stage was the sudden fall in current at the end of the pulse, associated with a formidable voltage transient. It is now known that the transient appears when the torus is clean, but a satisfactory explanation of it is not yet available.

Neutrons were first observed on August 30 when the current reached 120 k.amp. at an axial field of 160 gauss and a pressure of $\frac{1}{8}$ μ of mercury. The results of other investigations carried out on ZETA at that time have already been reported in *Nature* (January 25, p. 217).

Mr. Pease described some of the work undertaken since October. The machine has been improved in a number of ways: the insulation between the liners has been changed to polytetrafluoroethylene; irregularities have been removed from the axial magnetic field; the current pulse has been prolonged and condenser overswing prevented by short-circuiting the discharge when the condenser voltage drops to zero. Further modifications to obtain high currents are possible, but these involve removing protecting devices from the circuit and have not yet been attempted. It is hoped soon to replace the liner system by an improved one.

Mr. Pease concluded by describing the magnetic probe measurements; they confirm the densities and temperature obtained by other methods, and show that the gas is confined. They also show, however, that there is considerable mixing of the magnetic fields, and that the volts per turn at maximum current is not that associated with the resistance of the main channel.

When the discussion was resumed, with Sir John Cockcroft in the chair, Sir George Thomson complimented Sir John and the Harwell team on their achievement. He said the evidence that the observed neutrons have a true thermonuclear origin is strong. The temperature measurements themselves are sufficient to indicate that neutrons should be produced. If the neutrons are not thermonuclear, then the temperature measurements are in error and there must be a non-thermal neutron source which, by chance, gives the neutron yield predicted for the

incorrect temperatures; Sir George himself did not believe this.

Sir George went on to indicate the importance of electric fields in contained plasmas. The ions in ZETA have a large Larmor radius and would reach the walls in a very short time by scattering. The electrons, however, have a small Larmor radius and are firmly contained. The strong electrostatic correlation between the electrons and ions prevents the ions reaching the walls. The particle paths, being influenced by these strong electric fields, are trochoidal. These electric fields will get stronger and stronger as temperatures get higher.

Dr. A. A. Ware, of the Associated Electrical Industries Research Laboratory, gave an account of the work done there with a torus, Sceptre III, the dimensions of which are scaled down by about one-third compared to ZETA. The design of Sceptre III was originally based on theoretical proposals made by Liley at the beginning of 1957, but in view of the results obtained with ZETA a larger torus than that proposed by Liley was used. The main constructional difference between ZETA and Sceptre III is that the latter has the primary turns near to the torus. This construction reduces the leakage inductance and increases the gas current.

Much information has been obtained from inductance and resistance measurements made on the current and voltage wave-forms. In particular, the skin depth before the discharge leaves the wall is about 1/20th of the tube radius at high magnetic fields, and somewhat larger at lower magnetic fields. Measurements of inductance at peak current indicate that 90–100 per cent of the applied axial field is trapped by the contracting discharge.

Temperatures obtained from the neutron yield agree well with those from spectroscopic measurements. The variation of neutron yield with axial magnetic field shows a peak at that magnetic field which corresponds to the discharge being compressed to one-third of the tube diameter at maximum current. This is to be expected, since the heating of the gas increases with compression, but the discharge becomes unstable if it is compressed too far. Unlike the results obtained with ZETA and Perhapsatron, a maximum with respect to pressure has been found in the neutron yield. Neutrons appear shortly after the discharge has left the wall and the yield rises to a maximum around peak current. Neutrons continue to be emitted into the second half-cycle of the discharge.

Dr. Ware then referred to the comparison of the results with the theoretical predictions of Liley. Results obtained recently have tended to spoil the good agreement reported earlier[1]. The main discrepancies are: the skin-depth is small, a higher temperature is predicted than is observed, there are indications that the electron temperature is less than the ion temperature, and the temperature, as calculated from the neutron yield, does not vary in the right way through the pulse.

Dr. K. W. Allen (Atomic Weapons Research Establishment, Aldermaston) gave an account of work at Aldermaston on straight-tube discharges of short duration, the object of which is to heat the ions directly rather than by way of the electrons. A collapsing current sheath is set up which accelerates radially, the electrons by the pinch effect, and the ions by the electrostatic action between them and the electrons. In this way ions and electrons acquire equal velocities so that the ions get virtually all the energy. Rapid heating requires large sheath velocities and hence minimum circuit inductance. This has been achieved by using an arrangement of parallel spark gaps to discharge the condenser bank so that the inductance is that of a single unit divided by the number of units. A circuit has been devised such that all the gaps can be fired in less than 10^{-8} sec.

Neutrons were first observed at the rate of 10^4–10^5 per pulse on Christmas Eve, 1956. Typical conditions are: bank voltage 9 kV. and pressure 30 μ of mercury in a silica discharge tube 28 cm. long and 15 cm. in diameter. The initial rate of rise of current is 5×10 amp./sec., which is three times faster than that reported by Artsimovich and half that obtained at Los Alamos. Other workers in the field have reported that the neutron yields are very sensitive to impurities and magnetic fields, but this has not been confirmed at Aldermaston, where the yield has been found to be reduced only by an order of magnitude by 5 per cent of nitrogen or by 1,000 gauss axial magnetic field. Artsimovich reported that a few hundredths of a per cent of oxygen, argon or xenon quenched his yield, and Colgate reported that an axial field of 100 gauss reduced his neutron yield by four orders of magnitude. These results suggest that the mechanism responsible for the neutron production in the Atomic Weapons Research Establishment apparatus is different from that proposed by Colgate.

Dr. Allen concluded by saying that a larger condenser bank is under construction. It is hoped that this bank will give rise to a larger neutron yield which will permit a more detailed investigation of the mechanism of neutron emission.

Dr. J. D. Jukes, of Harwell, presented some comments of Dr. R. J. Bickerton and Dr. H. London (Atomic Energy Research Establishment, Harwell) on the scaling laws for stabilized discharges. Dr. Jukes said that, since various groups are seeking to produce thermonuclear temperatures in stabilized pinched discharges, it is desirable to compare similar experiments and to attempt to forecast the requirements of an economical reactor working on this principle by the use of scaling laws.

It can be shown that if I, the total current, and N, the line density, are kept constant, then the characteristic times of the various physical processes which determine the growth and life-time of the pinch, such as ohmic heating, relaxation, field diffusion and radiation cooling[2], are all proportional to R^2, where R is the tube radius. Furthermore, the temperatures are the same at corresponding phases of the life-time of the discharge.

The stable life-time is proportional to R^2, while the neutron yield Y during this time is proportional only to the axial length, and the fraction of nuclear burn-up is independent of size. The radiation flux to the walls per pulse is proportional to R^{-1} and the rate of rise of current required to trap the magnetic field is proportional to R^{-2}. This indicates that there is some engineering advantage in using a large apparatus.

Table 1 compares results of three experiments in the light of these scaling laws. The quantity nt, where n is the density and t the containment-time, is a measure of the efficiency of a reactor, as pointed out by Lawson[3]. This quantity must be increased to 10^{14} at least, and T to 4×10^7 °K. to make a device operating on the deuterium–tritium mixture produce net power. Extrapolating from ZETA this would require a current equal to or greater than 10^6 amp.

Table 1

	ZETA	Sceptre	Perhapsatron S3
I (k.amp.)	180	160	200
R (cm.)	50	15	2×5
N (cm.$^{-3}$)	6×10^{15}	8×10^{16}	6×10^{16}
t (sec.)	10^{-3}	10^{-4}	6×10^{-6}
L (cm.)	10^{3}	360	200
Y (neutrons)	10^{6}	10^{5}	2×10^{5}
tR^{-2}	4×10^{-7}	5×10^{-7}	8×10^{-7}
YL^{-4}	10^{5}	3×10^{5}	2×10^{5}
nt	10^{11}	10^{11}	10^{11}

Dr. W. B. Thompson (Atomic Energy Research Establishment, Harwell) gave a theoretical analysis of the operation of ZETA on the assumption that it produces a stable configuration very like that described by Dr. R. J. Tayler[4] later in the discussion, and that most of the heating is Joule heating. The process can be described as occurring in three phases. In phase 1 (0 to 100 μsec.) the gas is ionized, heated and a current skin formed, separating the axial and azimuthal fields; in phase 2 (100 μsec. to 1·5 msec.) the gas is compressed; and in phase 3 (1·5 msec. to end of pulse) the current skin slowly widens, the axial and azimuthal fields interpenetrate and eventually the plasma loses its stability.

Dr. Thompson showed that at the end of phase 1 the current sheath is 5 cm. thick, the temperature is $1 \cdot 5$–$2 \cdot 0 \times 10^{5}$ °K., and there is approximate thermal equilibrium. In phase 2 three processes are important: adiabatic compression takes the temperature to about 10^{6} °K., Joule heating adds about another 2×10^{6} °K., and finally shock heating may add a further 5×10^{5} °K. Thus, at the end of phase 2 the temperature is 3–$3 \cdot 5 \times 10^{6}$ °K. During phase 3 there is little further heating and the discharge loses stability. On the basis of Dr. Tayler's calculations (described later in the discussion) the discharge life is 1–2 msec.

Dr. Thompson concluded by saying that the picture he described is not in disagreement with the observations made on ZETA, with the exception of the magnetic-probe measurements. Magnetic probes, however, considerably disturb the discharge and thus may not give a good picture of what happens in their absence.

If the probe results are meaningful and applicable, then some other theoretical model will be needed. An example would be a turbulent but confined plasma, which would make well-mixed fields and high temperatures quite intelligible, but there is little experimental evidence for such a system.

Dr. R. J. Tayler (Atomic Energy Research Establishment, Harwell) spoke of the stability of mixed-field systems. He said that earlier work has shown that there should exist stable configurations of a pinched gas discharge if the currents flow entirely on the plasma surface. Dissipative processes will result in an interpenetration of magnetic fields. An estimate has now been made of the amount of penetration that can occur before stability is lost. A variational method developed at Princeton has been used, and with simplified profiles for the mixed fields, calculations have been made with a succession of trial functions. Results obtained for trial functions with between one and four arbitrary parameters show a change of only 5 per cent in the amount of penetration that can occur. For one initial configuration for which results have been obtained, the field can penetrate through about 15 per cent of the discharge radius. As there is no initial axial magnetic field outside the discharge in this configuration, the estimated duration of stability obtained from it is likely to be optimistic.

These results suggest that a stabilized pinch in ZETA can have a life-time measured in milliseconds. However, it is not clear from the measured magnetic field profiles that a stabilized pinch of this type actually occurs in ZETA.

Dr. Tayler concluded by giving an account of the article on "Co-operative Phenomena in Hot Plasmas" by Prof. L. Spitzer that appeared in *Nature* of January 25, p. 221.

Dr. D. Gabor (Imperial College of Science and Technology) thought that the discrepancy pointed out by Prof. Spitzer is real, and that co-operative processes may well be responsible for the high ion temperatures observed in ZETA and in Sceptre III. It seems necessary to postulate two collective mechanisms: one for transferring energy in a locally ordered form from the electrons to the ions, and one for creating molecular disorder in the ion gas. It is easier to make a guess at the second mechanism, because this may well be essentially similar to that responsible for 'Langmuir's paradox' in low-pressure arcs[5]. In these arcs electrons exchange energy with one another very rapidly, not through collisions but through the intermediary of an oscillating boundary layer. This has a potential distribution and an oscillation frequency such that electrons diving into it and returning to the plasma emerge with a resultant gain, at the expense of those electrons which have dived into the sheath an odd number of half-cycles earlier and have delivered energy to the oscillations. There exists a similar boundary sheath in the pinched discharge, with the difference that—due to the magnetic field—the ions are outside. It is very likely that this sheath is the seat of oscillations with a frequency of about twice the gyromagnetic frequency of the ions, thus creating intense interchange of energy between the ions and an energy distribution which is very nearly Maxwellian.

It is more difficult to guess at co-operative phenomena which may transfer energy from the electron gas to the ions. In the ordinary arc there is no such exchange, as the plasma-frequencies of ions and electrons are widely separated, by a factor of about 60 in the case of deuterons. On the other hand, the Larmor frequency of the electrons in ZETA is of the same order (10^{9} cycles/sec.) as the ion-plasma frequency, so that a necessary (but by no means sufficient) condition of co-operative interaction by oscillations appears to be met. Dr. Gabor conjectured that even under conditions when the plasma boundary as a whole is stable, there may be growing helical waves in the electron gas, essentially at right angles to the magnetic field, and weakly coupled to the ion–plasma waves, which are approximately parallel to it. Weak coupling makes the energy pendulate between the two types of waves, but during the periods when the energy is in the ion gas this is converted into disordered, thermal motion by the first-mentioned mechanism, thus preventing its return to the electron gas. To check these hypotheses it may be of interest to look out for strong oscillations in the 1,000-megacycle range.

A. Gibson

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INTERPRETATION OF SCIENCE TO THE PUBLIC

By Sir LAWRENCE BRAGG, O.B.E., F.R.S.
Royal Institution, London

SIR ALEXANDER FLECK'S recent address to the general assembly of the British Association, and the editorial in *Nature* of January 4, will be warmly welcomed by all who are interested in the popularization of science. The new plans for enlarging its activities which the British Association has announced focus attention on the need for presenting science in the most effective way to the general public. I am writing about this subject because the experience and traditions of the Royal Institution may afford some useful pointers in our endeavour to interest the public in science. It was founded 160 years ago for the special purpose of interpreting science to the public, and apart from the prosecution of research in its laboratories this has continued to be its primary function. The art of popular lecturing might indeed be said to be its main subject of research.

One of the oldest and most honoured traditions is that a 'Discourse' should be profusely illustrated by experiments and demonstrations. A popular talk to laymen differs in many ways from a lecture to students or an address to a learned society. To a professional scientist the description of an experiment is often quite sufficient because his own experiences in the laboratory enable him to visualize it. But a lay audience has not this advantage. To the layman the difference between the description of an experiment and the actual witnessing of it is as great as the difference between looking at a foreign country on the map and visiting it; we grasp its geography in a far more vivid way when we have been to the place. One is struck again and again by the immense superiority, as judged by the effect on the audience, of a series of experiments and demonstrations explained by a talk over a lecture illustrated by slides. The Christmas Lectures to young people at the Royal Institution afford a good instance. It is surprising how often people in all walks of life own that their interest in science was first aroused by attending one of these courses when they were young, and in recalling their impressions they almost invariably say, not "we were told" but "we were *shown*" this or that. This impact of the witnessed experiment is all the more important because the popular talk is so dependent on emotional reactions. Students take notes, when attending a course, and colleagues listening to an account of research are alive to its bearing on their own work and see the printed account afterwards, so the context is more important to them than the manner of delivery. The final result of the popular talk is measured, however, by the extent to which the audience recalls it afterwards, and this fixation of the image is effected by arousing an emotional response of interest and thrill.

It is perennially surprising to find that it is the simpler experiments which have the warmest reception, especially if they can be done on a spectacular scale. The audience likes to see 'the works'. It is of little help to do something mysterious in a box which makes a pointer move over a scale. The simple experiment is effective because it can be grasped completely, and the members of the audience have the pleasurable feeling that it has become a part of their own experience. Again, there is an art in arranging an experiment so that it shall be æsthetically attractive. We instinctively recognize this when we say after a lecture, "That was a pretty experiment he showed". These may seem small points; but they are important. They may make the difference between a pleased and responsive audience, and one which is bored. If the listeners go away feeling that they have really understood what the lecturer was trying to explain, then they will come again for more. Non-scientists must not be left to feel that all science is too difficult to understand, otherwise they will shut their minds and think that science is for scientists only.

The other guiding principle of the popular lecture is that of starting with something with which the audience is thoroughly familiar in every-day life, and leading them further with that as a basis. The survey of the new country must be tied on to fixed points which are already in their minds. This is one of the most difficult tasks facing the popular lecturer. He may be honestly trying to avoid technical language; but it goes further than that. He has to put himself in the place of the intelligent layman and realize that ideas and experiences so familiar to him are unexplored country to his listener. This may seem to be a stressing of the obvious; but I venture to stress it because I have rather special opportunities to assess the effect of popular scientific talks, and they often pass completely over the heads of the audience because an otherwise excellent talk does not establish an initial *rapport* with the listeners' knowledge and experience.

There is nothing novel in these views, they are merely a recapitulation of the principles which have been preached and practised by all the great exponents of popular lecturing from Faraday onwards. It seems to me, however, that they have an important bearing in planning our strategy for a wider interpretation of science to the public. Merely talking and writing about the wonders of science can be very ineffective because the public is nowadays so bombarded with talks and articles on subjects of all kinds through so many channels. If our message is to be heard above the general noise-level, we must use the most powerful aid we have. The primary way to interest the public in art or music is to show pictures or arrange concerts, and lectures on art or music can only be appreciated when they are related to such first-hand impressions. The primary way to interest the general public in science is to show experiments and demonstrations, and so let them share in the thrill of understanding how things work, which is after all a good popular definition of science.

The staging of a demonstration lecture requires much organization. A simple experiment which takes a few minutes to show in the theatre may demand as many weeks of preparation. There must be a supporting background of technical staff, workshop, and demonstration apparatus of all kinds as well as a suitably fitted theatre, and if a worth-while proportion of the public is to be covered, it means a whole-time use of these facilities which is costly. The Royal Institution is now running a series of demon-

stration talks for boys and girls throughout the school year, in collaboration with the science teachers, at which large-scale experiments are shown which cannot easily be staged with school resources. An approximate estimate of the cost, based on the proportion of overhead charges allocated to the scheme, is £5,000 a year, which is defrayed by the generous assistance of industrial firms and other bodies which are interested in the venture. Some 16,000 tickets are issued annually. The young people come from an area which includes about one-quarter of the schools in England, and their keenness indicates that this is a very repaying way of using the resources. Some half-dozen such centres operating on a similar whole-time basis would cover most of Great Britain. Certain famous annual lectures sponsored by various bodies are by tradition the occasion for numerous carefully prepared experiments, and these lectures draw enthusiastic audiences. Television can, of course, be a powerful means of transmission; but the laboratory resources must still be there behind the scenes for providing the programmes. My contention, therefore, is that merely increasing the number of talks and articles about science will not get us very far in interesting the general public, that on the other hand one gets the most rewarding response if one stages shows of experiments for them, and that if their interest is first so aroused, they will be much more able to appreciate the talks and articles.

OBITUARIES

Prof. D. R. Hartree, F.R.S.

Prof. D. R. Hartree, who died suddenly in Cambridge on February 12 at the age of sixty, had been Plummer professor of mathematics in the University since 1946. Before that he had spent fifteen years in Manchester, first as Beyer professor of applied mathematics and later as professor of theoretical physics. He was elected a Fellow of the Royal Society in 1932.

Hartree was born in Cambridge in 1897 and was seventeen years old when the First World War broke out. He served in the Munitions Invention Department and held a commission in the R.N.V.R. When the War ended, he returned to Cambridge to take the Natural Sciences Tripos. He became a Fellow of his own College, St. John's, and later migrated to Christ's College, where he held a fellowship for a short time before going to Manchester.

Some of Hartree's earliest work was on the propagation of radio waves in a magneto-ionic medium, and he wrote several important papers on this subject. His principal contribution to physics, however, was the development of the method of self-consistent fields for the computation of atomic wave functions. Early applications of wave mechanics had been to simple atoms such as those of hydrogen and helium, but Hartree saw a way of obtaining, by numerical means, wave functions for heavier atoms. He exploited this method with great vigour, and a long series of papers appeared in which Hartree and his collaborators presented the results of calculations for a large number of atoms.

Hartree's work on atomic wave functions led him to a more general interest in numerical analysis, and this eventually became his major preoccupation. To him numerical analysis was above all a practical subject and he brought the same kind of enthusiasm to computing as others bring to practical work in a laboratory. He always liked to tackle a new problem, and anyone who brought a problem to him would, as like as not, be given practical help as well as advice.

Although Hartree enjoyed computing—a little while before his death he claimed that he had some 10,000 hours of personal computing to his credit—he appreciated very early the advantages which might be obtained from mechanical aids. He was responsible for introducing the differential analyser into Britain, and the machine which, at his instance, was installed in the University of Manchester in 1935 was used to solve a wide range of problems. During the Second World War the machine was occupied with military problems of various kinds including, notably, an investigation into the mode of operation of the radar magnetron.

When the War ended, Hartree visited the United States to study the developments then going on in automatic digital computers. His practical experience of numerical analysis enabled him to make a distinctive contribution in this field and he rapidly became accepted as an authority. He was much in demand as a lecturer, and a course he delivered at the University of Illinois in the autumn of 1948 was published in book form.

When Hartree arrived in Cambridge in 1946, development work on an electronic digital computer was starting in the Mathematical Laboratory. Hartree associated himself with this project with characteristic enthusiasm and the Laboratory owes much to him for encouragement, advice and practical assistance. He continued his close association with the Laboratory until his death and took a prominent part in the running of successive summer schools in programme design and in the teaching for the diploma in numerical analysis and automatic computing.

Hartree did much to advance the interests of the Cavendish Laboratory, where he had his office. In particular, he was largely instrumental in reorganizing and improving the teaching of mathematics to science students, and in establishing theoretical physics as an optional subject in Part 2 of the Natural Sciences Tripos. In addition to his scientific interests, Hartree was a very keen musician, and played an important part in the founding of the Faculty of Music in the University of Manchester.

Hartree will be greatly missed by his many friends. He was the kindest and most unassuming of men, and endeared himself to all who worked with him.

M. V. Wilkes

Dr. C. J. Davisson

Physicists all over the world, and especially those of the older generation, will have heard with deep regret of the death on February 1 of C. J. Davisson.

Davisson is known best for his work on the diffraction of electrons by metal crystals, which established for the first time the wave character of free electrons and proved L. de Broglie's relation $\lambda = h/p$ between the wave-length λ and the momentum p of an electron. In fact, this work was the outcome of a long series of experiments started for quite a different purpose.

Davisson, who was born in Bloomington, Illinois, on October 22, 1881, took his degree in the University of Chicago in 1908. R. A. Millikan was there at the time and claimed with pride to have directed him towards physics. Davisson then went to Princeton, where he took his Ph.D., and his first published paper appeared in 1909 from Princeton. In 1911, working under O. W. Richardson, then a professor at Princeton, he published another paper on the emission of positive ions from heated salts of the alkali ion and established that they are singly ionized atoms of the metal. In the same year he married one of Richardson's sisters. He then went as instructor to the Carnegie Institute of Technology. His connexion with the laboratory where his life-work was done started as a consequence of the First World War. In 1917 he went to what is now the Bell Telephone Laboratories, then part of the Western Electric Company's organization, to take up a temporary post; he stayed there until 1946.

His war-time work was concerned with the physics of the then new thermionic valves and vacuum technique. After the War he continued to work on the fundamental principles of thermionics and was allowed a free hand in choosing his researches to an extent unusual in those days in an industrial laboratory. He was the first to show experimentally that the variety of Richardson's formula, which in fact makes the electron follow the Fermi–Dirac electron statistics (not then discovered), is preferable to the original form. He also did work which turned out to be of considerable commercial importance on oxide-coated filaments.

Davisson's discovery of electron diffraction arose from a study of the scattering of electrons, and the production of secondary electrons, from metals. The first paper on nickel, with C. H. Kunsman, appeared in *Science* during November 1921. It described surprisingly sharp peaks of reflected electrons which Davisson considered could be explained by scattering (classical, of course) from two shells of electrons. In 1925, after studying some other metals, he returned to nickel. Until then, the experiments had all been on ordinary polycrystallic metals; but by an accident the specimen became oxidized and in restoring the surface by heating was changed into an aggregate of only a few crystals. There was a complete alteration in the pattern of scattering and the former theory had to be abandoned. He was inclined at first to replace it by one of 'transparent directions' of the lattice; but, partly as a result of a visit to the British Association meeting at Oxford, he came to believe that the effect was connected with the new theories of de Broglie and Schroedinger. As a result of a careful search, he and Germer found the first electronic beams in positions which could be explained in this way on January 6, 1927. The consequences of this work of Davisson and Germer are well known, but it is not always realized what a supreme experimental feat these early experiments were. The slow electrons of the order of 150 volts which were used are most difficult to handle, and if the results are to be of any value the vacuum has to be what would still be considered good, and was then quite outstanding. In fact, very few experimenters have since been successful with the method; much faster electrons are used when electron diffraction is employed for the study of surfaces.

In 1937 Davisson shared with me the Nobel Prize in Physics for this discovery.

In the 30's Davisson was interested in electron optics, and with C. J. Calbick could claim the discovery of the principle of the electrostatic lens. During the Second World War he worked on the cavity magnetron. In 1946 he retired from the Bell Telephone Laboratories on reaching retiring age, and was appointed research professor in the University of Virginia and lived at Charlottesville until his death. He leaves a wife, three sons and a daughter. Among many other honours he was awarded the Hughes Medal of the Royal Society.

Davisson was a man of exceptionally lovable character. Slight in build and with a hesitant manner of speaking, which made him seem shyer than he really was, he had a delightful sense of humour and was full of fun. Though he worked most of his life in an industrial laboratory, he was essentially an individualist. Yet he had the reputation of being very accessible in the laboratory and most helpful to all who consulted him. Among those who had close contact with him at the Bell Laboratories are Lee Du Bridge, Merle Tuve, Philip Morse and William Shockley.

GEORGE THOMSON

Mr. J. L. Baker

JULIAN LEVETT BAKER was, when he commenced in 1900, the first chemist appointed to a London brewery. Born in 1873, he had trained at Finsbury Technical College and had worked as a chemist for the Beetroot Sugar Association before brewing became his life-work.

From the first, he energetically studied the many possibilities of improvement in brewing which science could suggest at that time, and in the course of his work he published some fifty papers—a number of them inspired by his original studies on sugars. In 1905, he also published a concise and readable book on "The Brewing Industry".

In 1904 he was joined by Henry Everard Hulton, and the two engaged in the work of the brewery and in numerous scientific studies. It was a memorable collaboration which lasted for thirty-four years until Hulton's death—at which time Baker wrote: "Throughout the long years no shadow was ever cast over an enduring friendship".

Indeed, all Baker's pursuits created something of a record for length. He was an original member at the formation of the Institute of Brewing in 1904 and was a vice-president for forty years (1918–58). He was a scientific editor for forty-two years—first of *The Analyst* (1907–20) and then of the *Journal of the Institute of Brewing* (1920–49); while his service with Watney, Combe, Reid and Co., Ltd., lasted for forty-six years.

His merits were recognized by the award in 1948 of the Horace Brown Medal of the Institute of Brewing.

Baker's interests and energies were not confined to brewing aspects of chemistry, for, besides his editorship of *The Analyst*, he was honorary secretary of the London Section of the Society of Chemical Industry from 1903 until 1909 and its chairman in 1919.

In his retirement, Baker, lovingly tending his garden, outlived his contemporaries to recall to later generations with graciousness and charm something of the marked individuality and wide culture which a band of young London scientists had displayed so outstandingly in the early part of this century.

He died on January 29. He is survived by his two sons and his daughter.

L. R. BISHOP

NEWS and VIEWS

Royal Geographical Society : Medals and Awards, 1958

H.M. The Queen has approved the award of the Royal Medals of the Royal Geographical Society as follows : *Patron's Medal :* Dr. Paul A. Siple, U.S. Antarctic Expedition, scientific leader at the International Geophysical Year's South Pole Base (the "Amundsen–Scott" Base), 1957–58, for contributions to Antarctic exploration and research ; *Founder's Medal :* Sir Edmund Hillary, leader, New Zealand component, Commonwealth Trans-Antarctic Expedition, for Antarctic and Himalayan exploration.

The Council of the Royal Geographical Society has made the following awards : *Special Gold Medal :* Dr. Vivian Fuchs, leader, Commonwealth Trans-Antarctic Expedition ; *Victoria Medal :* Prof. Roberto Almagià, professor of geography, University of Rome, for contributions to geographical research ; *Murchison Grant :* Prof. E. G. Bowen, Gregynog professor of geography and anthropology, University College of Wales, Aberystwyth, for studies on the geography of Wales ; *Back Grant :* Ingénieur-en-Chef Bertrand Imbert, leader of the International Geophysical Year French Antarctic Expedition 1957–58, for contributions to Antarctic exploration and research ; *Cuthbert Peek Grant :* Dr. Peter H. Davis, lecturer in botany, University of Edinburgh, for botanical exploration in south-east Turkey ; *Gill Memorial :* Dr. M. J. Wise, Cassel reader in geography, London School of Economics, for studies in economic geography ; *Mrs. Patrick Ness Award :* Captain A. G. Bomford, R.E., chief surveyor, South Georgia Survey, 1955–56.

The last occasion on which a Special Gold Medal was awarded by the Society was in 1910 when it was given to Robert E. Peary, of the United States Navy, the first man to reach the North Geographical Pole ; previous British recipients were Captain Robert Falcon Scott, R.N. (in 1904), and Sir Ernest Shackleton (in 1909).

Fourth Artificial Earth Satellite (1958β)

As part of the scientific programme of the International Geophysical Year, another artificial Earth satellite was launched by means of a 'Vanguard' rocket of the United States Navy from the testing ground at Cape Canaveral, Florida, on March 17. The new satellite, named 1958β, consisting of an aluminium sphere 6·4 in. in diameter and of mass 3·25 lb., is revolving in an eccentric orbit, varying between 400 and 2,500 miles above the Earth's surface, in a period of 135 minutes. The inclination of the orbit to the ecliptic is at present unknown. The satellite is equipped with two radio transmitters ; one, powered by chemical batteries, broadcasting on a frequency of 108·00 megacycles per second ; the other, powered by solar batteries, operating on 108·30 megacycles per second. The third stage of the 'Vanguard' launching vehicle, a rocket some 4 ft. in length with a mass of about 50 lb., which ejected the sphere from its nose when it attained orbital velocity, is also circling the Earth about a mile from the satellite. It is hoped that this satellite will continue in its orbit for a number of years and afford excellent opportunities for continued observations of value to geophysicists.

Brewing Industry Research Foundation : Sir Ian Heilbron, D.S.O., F.R.S.

Sir Ian Heilbron, who is retiring from the post of director of the Brewing Industry Research Foundation, Nutfield, Surrey, has held many distinguished academic appointments. He was professor of organic chemistry in the Imperial College of Science and Technology, London ; and also for some years a member of the Advisory Council of the Department of Scientific and Industrial Research, of which he became chairman in 1953. At the time of his appointment to the Foundation in 1948, the brewing industry had decided to start a central research station, and it fell to Sir Ian to select the site, and to adapt Lyttel Hall, the Victorian property chosen at Nutfield, to meet the requirements of up-to-date research laboratories. This he did with conspicuous success and the Foundation is now a notable example of a modern research station. Sir Ian's reputation as a scientist and in the sphere of research, in which he had rendered distinguished service on a number of departmental and national committees, enabled him to collect around him a band of first-class scientists, who quickly put the Foundation into the top rank of brewing research bodies in Europe, and extended its reputation to the United States. Not the least of Sir Ian's achievements has been that he has succeeded in keeping constantly before his staff, and before the industry, the ultimate practical applications of the fundamental research carried out at the Foundation.

Dr. A. H. Cook, F.R.S.

Dr. A. H. Cook has been appointed director of the Brewing Industry Research Foundation at Nutfield at the age of forty-six in succession to Sir Ian Heilbron. Dr. Cook gained the Ph.D. degree for research in organic chemistry at the Imperial College of Science and Technology, London, and after a further period of study at Heidelberg, worked for some time in the field of dyestuff chemistry with Imperial Chemical Industries, Ltd. In 1937 he joined the staff of the Imperial College where, with Sir Ian Heilbron, he did notable work on the chemistry of penicillin. In 1946 he was appointed reader and assistant professor of organic chemistry at the Imperial College, and in 1951 he was elected to Fellowship of the Royal Society. Since 1949 he has been assistant director of the Brewing Industry Research Foundation under Sir Ian Heilbron.

The World and the Observer

In his broadcast talk, "The World and the Observer", printed in *The Listener* of February 6, Lord Russell investigates the relation between our sensory experiences of material things and the theories of the natural sciences, in particular of physics, concerning the ultimate nature of physical reality. Primary particles such as electrons cannot be perceived, and are quite unlike the sort of objects which we can perceive. Russell's approach to the problem draws on physics, physiology, psychology and logic. His thesis may be summarized in three points. First, the entities dealt with by mathematical physics are "constructions composed of events and taken as units for the convenience of the mathe-

matician". Secondly, everything that is directly perceived is private to the perceiver; it is an occurrence in the brain of an individual. Thirdly, these occurrences are the effects of external causes, the real nature of which cannot be certainly known. As Russell says, these doctrines have not met with great favour from other philosophers. The short space of a broadcast talk is scarcely sufficient to deploy all the arguments and qualifications necessary for a systematic defence of such general and far-reaching theories. One sentence is rendered unintelligible in *The Listener* by the misprint of 'casual' for 'causal', and 'precept' for 'percept'.

The Isaac Newton Telescope and the Navy Estimates

QUESTIONED in Committee of Supply on the Navy Estimates under Vote 6 (Scientific Services) in the House of Commons on February 13 regarding the stoppage of work on the Isaac Newton telescope at the Royal Greenwich Observatory, Herstmonceux, the Civil Lord of the Admiralty, Mr. T. G. D. Galbraith, agreed that the telescope, the purpose of which is to extend the range of astronomical and astrophysical research, is of the greatest importance to the nation. Its cost is being shared between the Admiralty and the Treasury, but after £28,000 had been spent on primary design and study, of which £14,000 had come from the Admiralty, the project has now been suspended in view of the present financial stringency. Mr. Willis said that last year the expenditure was £30,000 and in the previous year £51,000, and he suggested that the stoppage gave point to the recent criticism of the Committee of Public Accounts. Mr. Galbraith said he could not, off-hand, confirm or deny these figures. As regards the International Hydrographic Bureau, while, as stated by Mr. Willis, in the past three or four years sales of charts had increased to 350,000, the 45 per cent increase in income was due partly to increases in price. The reduction in staff was due to the general need to economize. On the expenditure for scientific services generally, in the past two years the numbers of scientific and experimental personnel and of ancillary staff have fallen substantially, and research and development contracts have also generally decreased. The cuts, he said, had not affected important work like the development of nuclear propulsion, on which the Admiralty is working in very close co-operation with the Atomic Energy Authority.

Foundation for Mutual Assistance in Africa South of the Sahara

IN a written reply to Colonel T. V. H. Beamish in the House of Commons on March 10, Mr. Ian Harvey, Joint Under-Secretary of State for Foreign Affairs, said that he had represented the United Kingdom at the Conference at Accra on February 19 and 20, when the Foundation for Mutual Assistance in Africa South of the Sahara was inaugurated. The member governments were those of Belgium, France, Portugal, the United Kingdom, the Federation of Rhodesia and Nyasaland, Ghana, Liberia and the Union of South Africa, but other countries and territories in Africa south of the Sahara would be eligible for technical assistance under the scheme. A United States observer had been present at the Conference, but there was no commitment on the part of the United States Government. The technical assistance would take the form of the supply of experts, advisers and instructors to countries in the region, the training of personnel from the region and the supply of equipment for training purposes, and it would be arranged unilaterally following the practice of the Technical Co-operation Scheme under the Colombo Plan. The small secretariat of the Foundation would act as a clearing house between Governments for requests for assistance and offers. The House of Commons would be asked to approve new expenditure of £20,000 by the British Government for such assistance during the next financial year.

Chemical Engineering in a University

PROF. P. V. DANCKWERTS, recently appointed to the chair of chemical engineering science in the Imperial College of Science and Technology, London, delivered his inaugural lecture entitled "Chemical Engineering in a University" on November 12. He emphasized that a university does not turn out chemical engineers and that its function is to explain to students what a chemical engineer may have to do, to teach them the basic principles on which chemical engineering depends and how to apply them and, above all, to train them to think for themselves. By way of illustration he discussed several chemical engineering processes broken down to show their complicated nature, and the scientific subjects with which it was necessary to be acquainted before trying to build them up. Since, moreover, the practice of chemical engineering is often in advance of the theory, it is imperative for a university to maintain the closest possible contact with industry. Dealing with the difficult question of research, Prof. Danckwerts maintained that, although desirable, research is not an essential part of university training. Certain types of research, such as process development, are, in many cases, unsuited to a university laboratory, owing to the difficulty of working on a sufficiently large scale; but there is a wide and important field in "trying to get a deeper insight into the mechanism of industrial processes by looking at them with a scientist's eye". Examples of possible problems followed, and of particular interest were those dealing with 'scaling up'. Prof. Danckwerts did not venture to tackle the most difficult problem of all: Where is the teaching staff to come from? But perhaps he regards this as outside the scope of his subject. As the first of a series of inaugural lectures to be published by the Imperial College, Prof. Danckwerts's address has now been printed ("Chemical Engineering in a University": Inaugural Lecture by Prof. P. V. Danckwerts. Pp. ii+10+4 plates. London: Imperial College of Science and Technology, 1958. 5*s*.), and will be found illuminating and suggestive by all interested in engineering education and its products.

Palaeontology

EARLY in 1957, a group of British palæontologists met in London to discuss the formation of an association to further the study of palæontology by the holding of meetings in London and various other centres, and the publication of a journal. The inaugural meeting of the Palæontological Association was held in February 1957, and we now welcome the new journal, *Palæontology*, which made its appearance last November and is to be published half-yearly. The journal is crown quarto in size, and Vol. 1, Part 1, contains eighty-six pages and fourteen

collotype plates, the production being of the high standard associated with the Oxford University Press. Contributions will, it is hoped, cover all aspects of palæontology, and included in the first number are articles on the true *Rhynchonella*, Upper Viséan goniatites, the type-species of *Girvanella*, the Cretaceous ammonite *Leymeriella*, *Encrinurus multisegmentatus* and its allies, and descriptions of a new genus of foraminifer and a new Tertiary calcareous alga. The membership subscription is two guineas annually (the price per part of the journal is 35*s.* or 5.20 dollars). The president of the Association is Dr. R. G. S. Hudson, and the treasurer Dr. W. S. McKerrow, University Museum, Oxford.

Evolution

It has been said that the greatest and most enduring monument to the late Dr. O. G. S. Crawford is the journal *Antiquity*, which he inaugurated thirty-one years ago and edited up to the time of his death. He planned his last number, which appeared posthumously in December 1957 (pp. ii+185–257+5 plates. London: Phoenix House, Ltd., 1958. 10*s.* 6*d.* net), as a special evolution number to commemorate the Darwin–Wallace centenary, and it is particularly noteworthy for its frontispiece of a hitherto unpublished sketch of Charles Darwin made in 1855. The number includes a concise and authoritative summary of human evolution by R. Singer, a well-reasoned statement by K. P. Oakley on the evidence for the earliest stages of tool-making, and short articles by the late Prof. Gordon Childe on "The Evolution of Society", and by L. A. White on "Evolution and Diffusion". Another essay, highly appropriate because of its relation to some of Darwin's own studies, is by R. J. C. Atkinson on "Worms and Weathering"; it is the aim of this essay to direct attention to disturbances of the soil produced by worms which, unless taken into careful account, may tempt archæologists to base conclusions on what is really a falsified stratigraphy.

Cleaning Inscriptions

The *Museums Journal* for December 1957 includes an article by Dr. David Smith concerning the cleaning of inscriptions and sculptures in sandstone. The objects comprised altars, military inscriptions, tombstones and sculptures of the Roman and Anglian periods previously housed in the Black Gate Museum in Newcastle upon Tyne. They were cleaned in preparation for exhibition in the new museum erected by the University of Durham. The first step was to remove the surface dirt with a gentle spray of water directed from a rubber hose. For some of the worst cases this was the maximum treatment that the stone could stand. In other cases experiments were conducted with household detergents, dilute hydrochloric acid and fibre scrubbing brushes with hot water; and steel wire scrubbing brushes were also used with much success. This may seem a drastic method, but providing the surface of the stone is sound and the work is done under a constant flow of hot water, it has been found that no harm results. Each stone when completely cleaned was washed and maintained at a fairly constant temperature of 55° F. in another room. After about three months, a colloidal solution of silica in alcohol (ethyl silicate) was applied. This reinforced the natural bond of the stone, hardening it and reducing its porosity without completely sealing it or affecting it in any other way.

Remanent Magnetism

Study of the natural remanent magnetism of rocks is becoming a familiar method for determining the direction of the Earth's magnetic field at past stages in its geological history, and has led to interesting speculations about the wandering of the geomagnetic poles or of the continents themselves. An alternative use for measurements of remanent magnetism, namely, the determination of the temperature of formation of pyroclastic deposits, is described by Shigeo Aramaki and Syun-ito Akimoto in the *American Journal of Science* (**255**, 619; November 1957). The method depends on the fact that if all the fragments composing a deposit of this kind maintained a temperature above the Curie point of the constituent ferromagnetic minerals during emplacement, and cooled below this point only after coming to rest, their thermo-remanent magnetism must show a uniform orientation parallel to the geomagnetic field at the time and place of cooling. If, on the other hand, the fragments had already fallen below the Curie point before settling, the natural remanent magnetism of the fragments will have random orientations. Using this criterion, the authors have been able to distinguish between *nuée ardente* deposits and mud-flow deposits associated with several Japanese volcanoes and formed at various periods from recent historical to late Pleistocene times.

Wilmot Breeden Fellowships

The Wilmot Breeden Group of Companies, the manufacturing interests of which include motor-vehicles and gas-turbine components, hydraulics and electronics, are sponsoring two fellowships, each worth £1,000 per annum, one at the University of Birmingham and the other at the Birmingham College of Technology. Successful candidates will divide their time between the University or College and the Company. The fellowships will be advertised every year in March, in odd years in association with the University and in even years in association with the College. Each fellowship will normally be held for a period of two years. Candidates, who should have an honours degree of a university in the British Commonwealth, a diploma in technology, or an equivalent qualification, should normally have had two or three years experience of research or industry. An application for a fellowship should outline a two-year programme of research on which the candidate seeks to work. Inquiries can be addressed to the Secretary, Wilmot Breeden (Holdings), Ltd., Amington Road, Birmingham 25.

Society of Analytical Chemistry: Officers

The following officers of the Society for Analytical Chemistry were recently elected for the forthcoming year: *President*, J. H. Hamence; *Past-Presidents serving on the Council*, D. W. Kent-Jones, J. R. Nicholls, G. Taylor and K. A. Williams; *Vice-Presidents*, N. L. Allport, R. C. Chirnside and A. A. Smales; *Honorary Treasurer*, A. J. Amos; *Honorary Secretary*, R. E. Stuckey; *Honorary Assistant Secretary*, S. A. Price; *Ex-Officio Members of Council* (chairmen of Sections): A. N. Leather (North of England Section), Magnus A. Pyke (Scottish Section), S. Dixon (Western Section), R. Belcher (Midlands Section), D. F. Phillips (Microchemistry Group), R. A. C. Isbell (Physical Methods Group) and S. K. Kon (Biological Methods Group).

American Phytopathological Society

THE American Phytopathological Society is celebrating its fiftieth anniversary this year, and a series of outstanding symposia by internationally-known scientists is being presented at the meeting of the Society with the American Institute of Biological Sciences in August. A book of approximately 1,000 pages entitled "Plant Pathology—Problems and Progress 1908–1958", containing more than sixty symposium papers, will be published by the American Phytopathological Society in the winter of 1958. The papers in this volume are primarily basic in content and apply to many phases of the biological and physical sciences. Further information can be obtained from the American Phytopathological Society, P.O. Drawer 1106, New Haven 4, Conn.

University News : Birmingham

PROF. F. H. GARNER'S tenure as professor of chemical engineering and head of the Department of Chemical Engineering has been extended for two years to 1960. The title of reader in mechanical engineering has been conferred on Dr. B. N. Cole, senior lecturer in mechanical engineering; and the following appointments have been made: Dr. R. D. Peacock and Dr. J. Burdon, to be lecturers in chemistry; T. R. Bott, to be lecturer in chemical engineering; D. I. Rees, to be J. Lyons research fellow in analytical chemistry; Dr. D. A. Heath, to be lecturer in pathology; and W. Marsh, to be lecturer in orthodontics.

The new Chemistry Block (the first stage of which should be completed in 1960) is to be named the Haworth Building to commemorate the late Sir Norman Haworth, who was Mason professor and director of the Department of Chemistry in the University during 1925–48.

London

DR. S. R. SPARKES, reader in civil engineering at the Imperial College of Science and Technology, has been appointed to the chair of engineering structures tenable at that College; and Mr. G. R. Hall, acting principal scientific officer at the Atomic Energy Research Establishment, Harwell, has been appointed to the readership in nuclear technology tenable at the Imperial College.

Announcements

PROF. C. A. COULSON, Rouse Ball professor of applied mathematics in the University of Oxford, will deliver the 1958 Eddington Memorial Lecture in Cambridge, at the Arts School Lecture Theatre, Bene't Street, on April 21 at 5 p.m.; his subject will be "Science and the Idea of God".

A SYMPOSIUM on Molecular Structure and Spectroscopy will be held in the Department of Physics and Astronomy, Ohio State University, during June 16–20. There will be discussions of the interpretation of molecular spectroscopic data as well as methods for obtaining such information. Accommodation will be available for those who wish to reside on the campus during the meeting. Further details can be obtained from Prof. R. A. Oetjen, Department of Physics and Astronomy, Ohio State University, Columbus 10, Ohio.

THE 1958 Microscopy Symposium, the fifth of these symposia, will be held in Chicago during June 9–12 and will include sessions on instrumentation, electron microscopy, X-ray microscopy, autoradiography, medical and biological applications, fibres and plastics, forensic microscopy and structural materials. Exchange of ideas and techniques are encouraged in an informal atmosphere. Further information can be obtained from W. C. McCrone, jun., Walter C. McCrone Associates, 500 East 33rd Street, Chicago 16, Illinois.

THE Postal and Telecommunications Group of the Brussels Universal and International Exhibition is organizing an international conference on "Solid State Phenomena in Electronics and Telecommunications" to be held in the University of Brussels during June 2–7. Three major subjects will be treated at this conference: semi-conductors, magnetic materials and photo-sensitive and luminescent materials. Prospective authors of papers in those fields are invited to notify the organizers as soon as possible. Further information can be obtained from the Hon. General Secretary, Société Belge de Physique, Loverval, Belgium.

A CONFERENCE on "Noise", with particular reference to industry, organized by the British Occupational Hygiene Society, will be held at the Canterbury Hall, Cartwright Gardens, London, W.C.1, on April 1. The conference is open to non-members of the Society on payment of a registration fee of one guinea. Mr. T. E. Cawthorne (King's College Hospital) will discuss the physical disturbances produced by excessive noise; Dr. T. S. Littler (Wernher Research Unit on Deafness) will describe the techniques of measuring noise; Dr. A. E. Carpenter (Applied Psychology Research Unit of the Medical Research Council) will deal with the disturbance of mental performance caused by noise; and the suppression of noise will be dealt with by Mr. N. Fleming and Mr. W. C. Copeland (National Physical Laboratory). Further details can be obtained from the hon. scientific secretary, Dr. D. Turner, Associated Ethyl Co., Ltd., Ellesmere Port, Cheshire.

THE Non-destructive Testing Group of the Institute of Physics has organized a joint conference with the Société Française de Métallurgie, to be held in Paris during June 16–20. The theme is to be the utilization of physical properties for studying the relationships between the constitution, structure and service behaviour of metals, and there will be both French and English chairmen at each discussion session. The conference, for which there is no fee, is primarily intended for members of the Institute of Physics, but there will be limited accommodation for non-members. A parallel programme, including visits to Malmaison and St. Germain, is being arranged by the French Metallurgical Society, and members' ladies are expressly invited on this occasion. Those who wish to attend should apply, before March 24, on forms obtainable from Mr. L. Lawrence, Institute of Physics, 47 Belgrave Square, London, S.W.1, to whom all inquiries should be addressed.

ERRATUM. In the article by Dr. E. B. Kraus entitled "Recent Climatic Changes" in *Nature* of March 8, p. 666, the formula in the second paragraph is incorrect; for $y(n) = 100 \sum_{l=1881}^{n} \left(\frac{rl}{\bar{r}} - 1\right)$ read $y(n) = 100 \sum_{l=1881}^{n} \left(\frac{rl}{\bar{r}} - 1\right)$.

TECHNOLOGICAL EDUCATION IN BRITAIN

THE development, up to July 1957, of courses leading to the new Diploma in Technology is recorded in the first report of the National Council for Technological Awards, which was published on November 21*. At that date there were 965 students in thirty-seven courses leading to the new award. The report gives a full list of the courses now in progress which have been approved; these include applied chemistry, applied physics, applicable mathematics, applied pharmacology, applied biochemistry, aeronautical engineering, mechanical engineering, civil engineering, electrical engineering, and production engineering. Of 123 applications which had been received, 83 had been considered by July 31, 1957, and of these 49 were accepted and 34 rejected. The 49 accepted comprised 29 existing courses and 20 proposed new courses.

The report opens with a brief review of the events leading up to the establishment of the National Council, with its two boards of studies, one for engineering and one for technologies other than engineering. The constitution of the subject panels which deal with individual technologies is then mentioned and the procedure for the consideration of courses is described. The written submission from a college is considered by the appropriate subject panel, and, generally, the college is visited by members of the Council, the object of the visit being to inform the Council of the setting in which the course would be conducted, to meet the principal of the college and his staff, to see the equipment and accommodation available, and in general to enable the Council to form a better assessment of the position than could be done on the basis of the written application alone.

Several of the important features of the scheme receive comment in individual paragraphs of the report. Recalling that the standard of admission to courses is either five subjects in the General Certificate of Education, of which two appropriate to the course must be at Advanced Level, or a 'good' Ordinary National Certificate, the comment is made that the Council has not attempted to establish a definition of 'good' for this purpose, but has considered individually the proposals put forward by colleges. It is pointed out that there is also provision for the entry of students not possessing these qualifications but who have completed successfully a course of preliminary training.

Dealing with syllabuses, the report reiterates that, in all courses approved, the study of fundamental science and technology and their application provides a course of disciplined study fully equivalent to that leading to a university honours degree. The interesting comment is made in this section that in relation to some of the courses which the Council has rejected the weakness has been one, not of inadequate syllabus content, but of overcrowding the syllabus, resulting in an excessive number of hours in the week assigned to formal classes.

The importance of providing a reasonable amount of time for private study is emphasized and it is remarked that few colleges have adequate space available for this purpose. This, it is implied, is associated with inadequate library facilities—a situation which is being rectified.

Specific reference is made to the 'project' as an important part of the work of the course in the final year, and it is suggested that the project may well serve as a link between the industrial training and the subjects studied. Liberal studies which might serve to give the student some appreciation of the social and economic significance of the technology he is studying are expected to form part of the curriculum and should constitute subjects which are examined. It is recognized that the best methods of incorporating such liberal studies are still very much a matter of experiment, and an appreciative reference is made to the serious way in which colleges are tackling the pioneer work to be done in this matter.

In regard to staff, special reference is made to the report of the Special Committee on the Supply and Training of Teachers for Technical Colleges. Of particular urgency are the matters of reducing the excessive teaching load carried by many teachers in technical colleges, the provision of adequate research facilities and of adequate research assistance and clerical assistance. Associated with these are several other recommendations from the report, among which may be mentioned the proposal that "senior members of industrial staffs should be given a special status in colleges permitting them to be closely identified with all the academic activities of the college" and the "appointment of representatives of industry and commerce to college Governing Bodies as a means of ensuring the effectiveness of the college's work and of helping to secure part-time staff from industry".

Another of the fundamental principles upon which the Diploma in Technology is based is restated in the report in the words: "The Council regard a course leading to the Diploma in Technology as a complete entity, the industrial training being just as much a part of the course as the academic study". Full and wholehearted co-operation between college and industrial firm is essential if works experience and academic studies are to be genuinely complementary. The view is expressed that such close and effective association may well be difficult to achieve when a number of small firms are involved, but its attainment is of the utmost importance as it constitutes one of the most valuable—and indeed unique—features of the scheme. Associated with this is the problem of maintaining an appropriate amount of study during the works periods.

The Council is confident that the substantial building programme now in progress will remove the majority of the many deficiencies in accommodation found in colleges. These deficiencies have in the main concerned amenities for student social activities, libraries, accommodation for project work and private study, and for research by staff. Residential accommodation for students is regarded as essential, but the initial absence of such facilities has not prevented the recognition of courses.

The main body of the report concludes with a statement that the Council has set up a special committee to make recommendations as to what post-graduate award the Council should offer. Six appen-

* National Council for Technological Awards. Report for the period December 1955 to July 1957. Pp. 20. (London: National Council for Technological Awards. 1957.)

dixes provide information concerning the governing body, boards of study, courses in progress which have been recognized and new proposed courses, together with notes about the subjects of courses approved and rejected. The last two appendixes state the examination arrangements and give the decisions of the Council concerning the minimum length of a sandwich course, and conditions for exemption from the first year of the course.

It is evident from the report that the response of technical colleges to the institution of the new award has been excellent. The relatively high rate of rejection for courses offered has been due to the insistence of the Council on the maintenance of fully adequate standards, a matter of paramount importance in the establishment of the standing of the new award.

Enrolment of approximately one thousand students in the courses recognized up to July of last year represents a substantial and encouraging start to a project which should make a great contribution to meeting the need of Britain for additional technologists. Although the appendixes to the report give considerable detailed information about the position, one vital factor is absent, namely, the distribution of students in relation to their method of satisfying the entrance requirements. In assessing the net gain, the real criterion is that of the sources from which the men are drawn, and it is to be hoped that in future reports this information will be presented.

SCIENCE EDUCATION IN MALTA

DURING Malta's tenth education week, which was organized by the Malta Union of Teachers during May 4–11, 1957, Prof. G. P. Xuereb, dean of the faculty of medicine and surgery in the Royal University of Malta, discussed the place of science in the Maltese educational system.

There is in Malta an acute realization of the fact that changes are necessary in the educational system. The social environment has changed, and educational methods have become unrelated to the social background. There must be an intimate and indissoluble connexion between the pattern of an educational system and the social order for which it is preparing the citizens.

One of the principal causes of the maladjustment between Maltese education and the country's needs lies in the fact that the new scientific knowledge, from which social changes have sprung, is incorporated very imperfectly in the educational system.

The majority of Maltese educated men and women are unaware of the way scientific discovery has shaped society in the past and of the way in which it might change it in the future. Inadequacy of science teachers is imperilling the introduction of the sixth forms in school. The Civil servant is having to face the administration of industry, trade development, public health, statistics, technical education, labour and social services created by forces that his education did nothing to lead him to understand. Everywhere there are improvized methods, because education has given no training.

Scientific studies are not providing the social stimulus and the intellectual training which they should. Judged by the actual hours of teaching, science is still a minor subject in general education. Many teachers still believe that science should not be taught as part of a general education—a belief disguised laconically by the statement that they wish to avoid "premature specialization".

The content of the science courses also leaves much to be desired: the course must be coherent so that everybody realizes the connexion between the successive parts of his science work. Instead of coherence there is a strong bias towards physics—and physics in its most arid form. Chemistry is not yet established. Biology, far from being taught as the natural history of animals and plants, is 'nature study' and is hurriedly jostled out of the curriculum when it arouses the inborn curiosity of youth. Science is being taught as a collection of laws and facts rather than as a constantly growing body of knowledge with social implications of vital importance. The social repercussions of science are relegated to a few isolated industrial applications. Briefly, science is presented as a dead body of facts, rather than as a living cultural and social force.

When these boys go to the Royal University, a course in general science is quickly prescribed for the lawyers, legal procurators and theologians of to-morrow. In this course the lecturers ensure that the students learn to believe the authority of the master or text-book and to reproduce the text when asked. These students are confined to the lecture room and are never referred to the laboratories.

The science courses are not much better themselves. Science students spend two years at the University acquiring the basic theoretical and practical knowledge which they should have more appropriately gathered in the secondary schools. Then, when they are ready to absorb some scientific method, they are passed on to the intermediate course in medicine, pharmacy, architecture and engineering. In medicine considerable time is devoted to introducing them to the sequence of hypothesis followed by experiment or observation and then analysis. Is it surprising that nearly 70 per cent of science students fall by the wayside and that only 30 per cent obtain a medical qualification?

In the past three years the University has taken certain steps to ensure that science is brought into closer relationship with the needs of life. A four-year course for Civil servants has been instituted leading to a diploma in social and political administration. Scholarships in biology, chemistry and physics have been provided, and three Maltese graduates are now at Oxford and London taking the honours school in these specialities before appointment to lectureships in science. The University of Malta will shortly be requiring matriculation in chemistry and in physics or biology from prospective science students. The University has drawn up plans for reform in the course of pharmacy. The libraries have been put on a scientific basis, science laboratories are now completed and funds have been secured for the building of a medical school. These measures are a beginning: they will contribute greatly towards bringing the Maltese educational system into line with a changing social environment.

MANAGEMENT DEVELOPMENT

AT a conference organized by the British Institute of Management at Harrogate during January 22–24, more than a hundred representatives from leading industrial organizations in Britain discussed the finding and development of managers for industry.

To open the conference, Mr. E. F. L. Brech, of Urwick, Orr and Partners, examined the motives which cause young men to choose a managerial career. The goal could be money, the idea of power, prestige, or a wish to cover up the man's sense of inferiority. Mr. Brech suggested that these reasons are inadequate. The essence of management is responsibility for a social task, and the better educated mind should be attracted to management with the offer of a satisfying social job rather than for the financial return.

He wondered whether the advertisements selling the idea of a big and lucrative future in management were carrots to draw in bright young men who would not be given a chance to get ahead. Many men now in top managerial posts should not be there, and firms should keep their eyes open and remove incompetent men before they got too far. The responsibility of management is to do the job efficiently and economically. Good human relations lead to good morale and so help to achieve this end.

Here, Mr. A. Brown, administration and personnel director, Mars, Ltd., suggested that all companies should adopt an appraisal system to ensure that all human resources in the company are looked at systematically at least once a year. The right man, the best man for the job, should be sought within and without the company.

If the company needs a 'go-getter', then he should be sought. If the company needs a systematic, conservative type, he should be looked for. Small and medium-sized firms must often rely on large firms to train managers who will either find inadequate or too distant scope in those firms and seek work in the smaller company with its close relationships and opportunity for wide experience.

When selecting men as managers there must be not only a job specification but also a full definition of the requirements of the job in relation to the organization, its personnel and future planned development. It is desirable to know who the man will work with. Is a systematic or intuitive approach required in the job as envisaged? These are the kind of facts which should be known but rarely are.

If the company has a natural successor for an appointment, it should dispense with elaborate selection methods. If the company is satisfied that it has the right man after examining all information and records, the selection should be made there and then.

Mr. W. W. Ferguson, of James Williamson and Son, Ltd., suggested that, to find the right man, the old Army custom of making men into acting temporary unpaid lance-corporals should be tried on the shop floor. In this way, argued Ferguson, it could be discovered whether the prospective foreman was a good leader and, by letting him work with a carefully chosen foreman, he could be seen in action. If he were satisfactory, all would be well; if not, he could be returned to his previous job.

This assumption was not generally accepted. One or two delegates supported Mr. Ferguson, but many others rejected his plan as impractical. Most of the delegates felt that in industry it was no easy thing to lower a man's status after raising it, and that the temporary foreman would suffer as he could have no real authority during his trial period.

In an informal discussion, the conference considered a suggestion that the end of National Service would be the end of an interlude which did much useful work in disciplining the nation's industrial recruits. More than one speaker claimed that industry could get along quite well without this service, and that it could undertake its own teaching of discipline and training of apprentices.

One of the general conclusions emerging from the conference was that management development is supported only by a representative, sincere and progressive slice of British industry. More than once voices were raised to ask whether top management—the policy makers and leaders of industry—was yet sufficiently convinced of its practical value.

Examples were given of firms with comprehensive management development programmes sympathetically encouraged by the men at the top: so were more cynical views that top management thought such programmes a waste of their valuable time.

T. H. Hawkins

ELECTRON MICROSCOPISTS

AT the fifteenth annual meeting of the Electron Microscope Society of America, which was held at the Massachusetts Institute of Technology on September 11, 1957, the president, Dr. J. H. L. Watson, head of the physics department of the Edsel B. Ford Institute for Medical Research (Henry Ford Hospital, Detroit 2, Michigan), gave a summary of the results and conclusions of a survey which he had recently conducted on the subject of what was expected professionally of a trained electron microscopist. The survey was made by questionnaire and letter addressed to two hundred electron microscopists and directors or supervisors of laboratories in which electron microscopy is used. One hundred and thirty-nine answers and opinions were received, but despite the diverse fields of application in the various institutions the replies showed a surprising unanimity.

A good laboratory attitude, a co-operative personality, scientific imagination, and patience, were the particular personal qualities required. Familiarity with the particular field of application, in addition to electron microscopy, was stressed, as was also the importance of ability to inform others clearly and concisely of the work in progress. No desire was expressed by the biologists or non-biologists for a difference in training of scientists for electron micro-

scopy in their respective spheres, except that the non-biological institutions mentioned that especial emphasis should be given to optical microscopy. The replies included eleven from sources outside the United States, but their requirements were almost identical with those from the industrial non-biological institutions, differing only in that 'skill in photography' was asked for in preference to 'knowledge of allied instruments'.

In almost all cases the duties expected of a trained electron microscopist were expected in a high order of perfection—not just to maintain the instrument, but to maintain it 'at its peak'—thus demanding a maximum of experience and training in the field and indicating a preference for postgraduate training.

Dr. Watson referred to the great shortage of research electron microscopists in the United States of America and stated that a strong plea could be made for graduate university courses in electron microscopy where men from diverse fields could be trained in the fundamentals of the subject and conduct electron microscopical research in their respective fields leading to postgraduate degrees.

S. WEINTROUB

FREEZING FISH AT SEA

A PRIMARY aim of the work at the Torry Research Station of the Department of Scientific and Industrial Research is the improvement in quality of fish as sold to the consumer. Particular attention has been paid to the problem of fish, principally cod, caught in Arctic waters, since the earlier part of the catch may be some twelve days old when it is landed. During the voyage the fish are stowed in holds in crushed ice, and it is obvious that the quality at landing could be markedly improved if the fish were quick-frozen soon after catching. "Report on an Experiment into the Freezing of Fish at Sea" (pp. 65+6 plates. London: White Fish Authority, 1957. 5*s*.) is a record of an extensive and impressive large-scale experiment to examine the practical feasibility of such a scheme of quick-freezing at sea. The technical developments were undertaken by the Department of Scientific and Industrial Research, and in the field work the White Fish Authority and the trawler owners gave full co-operation.

Five main phases of the technical work are fully described: (1) the experimental development in the Torry engineering laboratories of a suitable type of freezer for whole cod; (2) the sea-trials of a prototype freezer in the Station's motor fishing vessel; (3) collection and analysis of operational data on fourteen typical trawler trips to northern waters, to provide a design basis for a full-scale plant; (4) the construction and shore testing of the full-scale freezing plant and the conversion of a typical trawler, *Northern Wave*, to take the installation; (5) the results of the eight experimental voyages with the freezing plant in action.

The freezing plant has a capacity of about 8 tons of fish per day and roughly 23 per cent of all fish caught was quick-frozen. Freezing was effected by direct-contact plates through which refrigerant at − 40° F. was circulated, and the frozen blocks were stored at − 20° F. in a specially installed cold store.

Details are given of the marketing experience and reaction to the product, and the financial and economic implications are summarized. The scale of the experiment was not large enough to permit of firm conclusions on all these points, but the findings should be of marked assistance to trawler owners contemplating the adoption of freezing at sea. The outstanding feature of the experiment was the successful performance of the freezing equipment under all weather conditions, and the technical feasibility of the project was amply demonstrated.

CARTOGRAPHY

THE first volume of the official records of the first United Nations Regional Cartographic Conference for Asia and the Far East consisted of the report of the Conference. This has now been supplemented by a second volume*. Of this, the first 22 pages, or about one-sixth, consists of summary records of the discussions, which took as their starting point a recognition that "accurate maps are a prerequisite to the proper development of the world resources which in many cases lie in relatively unexplored regions", and that topographical mapping must necessarily precede geological or other specialized surveys. Delegates were concerned largely with the need of the less-developed countries for technical assistance in both survey and map compilation; and with the means by which publication of further sheets of the International Map of the World on the Millionth Scale might be encouraged. The "Technical Papers" follow and vary from an account of the status of publication of the International Map accompanied by communications on its future from various interested parties to descriptions of some of the most recent developments in survey methods.

The report on the "International Map of the World on the Millionth Scale"* is, except to the cartographically well informed, a most misleading document. It misleads particularly by its index map, which conveys the impression that the Map itself is well on the way to completion. This is far from being the case, and since it is important that scientists generally should be aware of the realities of the situation regarding this basic map of the world, it is well to recall the history of this project.

The original proposal for an international map of the world on a scale of 1 : 1 million (about 16 miles to 1 in.) was made by Prof. A. Penck at the International Geographical Congress in Berne in 1891. It

* United Nations Regional Cartographic Conference for Asia and the Far East, 15–25 February, 1955, Mussoorie, India. Vol. 2: Proceedings of the Conference and Technical Papers. Pp. x+133+2 maps. (New York: United Nations; London: H.M. Stationery Office, 1957.) 1.50 dollars; 11*s*.; 6.50 Swiss francs.

* International Map of the World on the Millionth Scale: Report for 1955 prepared by the Secretariat. Pp. iii+84+1 map. (New York: United Nations; London: H.M. Stationery Office, 1957.) 0.80 dollars; 5*s*. 6*d*.; 3.50 Swiss francs.

was recognized from the outset that the standard of the surveys from which it would be compiled varied greatly in different parts of the world, but it was felt that if the sheets were published in a uniform style and those features which had only been roughly explored were clearly distinguished, a very useful map would result. It was discussed at succeeding (4-yearly) conferences and by 1910 detailed specifications for projection, sheet lines, conventional signs and layer colouring had been drawn up, with the object of ensuring that sheets published by different countries should be as much a part of a uniform series as if they had been issued by the same publisher. By 1914 about a dozen sheets had been published. During the War several sheets were issued in styles closely approximating to that specified, but the Paris Conference of 1913 had ruled that sheets not completely conforming could not be regarded as part of the International Map, and in the first report of the Central Bureau of the Carte International du Monde (1921) two index diagrams were printed—one showing international sheets and the other sheets sufficiently similar to be conveniently used to supplement them. This distinction was confirmed by the conference of 1928 and firmly adhered to in the period between the Wars. Some of the most important contributions, including the American Geographical Society's series of sheets of Hispanic America, which was commenced in 1922 and completed in 1945, fall in the latter category, though they closely resemble International sheets.

In 1939 a map on this scale was again found to be most useful, and many sheets were prepared by military establishments. As time went on, they departed more and more from the specifications for the International Map and by the end of the War the position was very confused. In September 1953 the functions of the Central Bureau at Southampton were transferred to the United Nations Cartographic Office and an attempt was made to find out what had been published. The first result was an index diagram in World Cartography, Vol. 3. The report of the regional cartographic conference referred to above contains a reproduction of this diagram, which bears the date January 1957, though World Cartography, Vol. 4, which was published in the first half of 1956, and was devoted entirely to the International Map, contains a later diagram showing many more sheets, particularly in Asiatic Russia and Australia. It also contains most useful reproductions of the resolutions of 1913 and 1928.

The report for 1955 on the International Map of the World on the Millionth Scale (published in 1957) again shows more sheets, this time mostly of China. In both cases it is explained that the additional sheets are not necessarily newly published, but that their existence had not previously been reported to the United Nations. However, an inspection of the tabulated list of sheets reveals the fact that a great many, particularly of these additional sheets, are published by either the Geographical Section of the General Staff of Great Britain, or the Army Map Service of the United States, and are for "official use only, and not for sale or distribution". What is not revealed is the equally important fact that in general they depart considerably from the specifications for the International Map, though it is made clear that many of those of China are in black-and-white only and in the Chinese language. In fact, almost no reference is made to the distinction so carefully drawn in the years before 1939 between International and other sheets. Since the latter have become so very heterogeneous, some of them having little except projection and sheet lines in common with the International Map, the report is of little value as a guide to the present state of what used to be known as the Carte International du Monde. It would be a great pity if the challenging but very desirable and, one hopes, not unattainable objective of a homogeneous series of maps covering the whole world which for so long received the support of many distinguished geographers and surveyors should become obscured by a welter of second-rate publications, not all of which are even on public sale. A. M. Ferrar

INSECTS OF MICRONESIA

THE attention of students of the terrestrial Arthropoda in general is directed to a series of publications by the Bernice P. Bishop Museum, Honolulu, Hawaii, entitled "Insects of Micronesia". The geographical area covered includes practically all the oceanic islands of the north-western Pacific Ocean west of the 180th meridian and north of the equator. The chief island groups are the Marianas, Carolines, Marshalls and Gilberts, and the isolated islands are Ocean, Nauru, Wake, Marcus, the Bonins and the Volcano Islands. Altogether, Micronesia comprises some 2,400 islands under the administration of the United States, the United Kingdom and Australia. The former Japanese possessions, the Bonin Islands, Volcano Islands and Marcus, are now under United States mandate.

This series is based largely on the collections made between 1947 and 1953 by the Pacific Science Board (U.S. National Research Council) with the financial support of the U.S. Office of Naval Research, but much material was collected during the Second World War by American Service-men and previously by Japanese workers headed by Prof. Teiso Esaki, of the University of Kyushu. Since 1953 the work has been financed by the U.S. National Science Foundation, which has also assisted in the publication of the series.

"Insects of Micronesia" is planned to appear in twenty volumes which will be published at irregular intervals when ready. Vol. 1, which explains the scope and aims of the series as well as discussing the environment and natural history, was published in 1954 and is by Dr. J. Linsley Gressitt, entomologist at the Bishop Museum and leading spirit in this ambitious project. Vol. 2, published in 1955, is a comprehensive bibliography together with a subject index and is by Prof. Teiso Esaki, E. H. Bryan, jun., and J. L. Gressitt. Vol. 3 will be devoted to the Arachnida, Vol. 4 to the Crustacea, and Vols. 5–19 to the insect orders, followed by a summary in Vol. 20. Although none of these volumes has been completed, twenty-two parts have been issued to date.

This work is being undertaken by 115 specialist authors representing about fifty institutions in some thirteen countries, and is an example of the international effort required to deal with the vast assem-

blage of insects. Some idea can be gained of the increase in knowledge resulting from this publication by reference to the most recent issue, Vol. 7, No. 3, Heteroptera Aradidae, by Ryuichi Matsuda and Robert L. Usinger, in which the number of known species of this obscure family of fungus-feeding bugs is raised from thirteen to forty.

It is to be hoped that "Insects of Micronesia" will be successfully concluded in the not too distant future.

W. E. China

A TRAP FOR INSECTS BITING SMALL VERTEBRATES

By Dr. W. H. R. LUMSDEN

East African Virus Research Institute, Entebbe, Uganda

In estimating the biting insect attack on man the bait may be made to perform the work of catching, as in the 24-hr. catch system with human catchers[1], and so confusing factors are minimized. In the case of other animals, however, a difficulty arises. If men are used for catching the insects alighting to feed on an animal bait, it remains in doubt whether the insects are primarily attracted to the animal or to the men, only biting the animal secondarily. Conversely, some of the insects biting the men might be expected to bite the animal if it was alone. Previous attempts to resolve this difficulty with unattended traps baited with animals have employed traps with restricted inlets[2,3]. Such traps are very successful for some species of mosquito, less so for others, perhaps because species vary widely in their readiness to enter. Further, these traps do not retain all the insects entering them; a certain, probably variable, proportion escapes[3]. The essentials of an improved design appeared to be that access to the bait should be as open as possible and that insects attracted to the bait, and captured, should not be able to escape afterwards. Some reconciliation of these apparently mutually exclusive characteristics has been accomplished in the trap to be described, which samples at regular intervals the flying insects in the vicinity of a bait which is as nearly as possible freely exposed.

A first design of trap[4] employed a horizontal flow of air over the bait. Although it was successful its efficiency was considered likely to vary with changes in wind direction and velocity, and so it has been superseded by a vertical design (Fig. 1). A 'Vent-Axia' 12-in. fan is mounted on a framework of iron strip and rod over the centre of a platform of hardboard. The trap is quite open around the platform for a height of 15 cm. Above this, and up to the base of the fan assembly, a conical space is partially enclosed by truncated sectorial pieces of transparent cellulose acetate sheet between the edges of which are spaces about a centimetre wide. The bait, in the middle of the platform, is thus visible from all directions above the horizon except from directly and nearly directly overhead, and access of insects to it is practically unimpeded. Above the fan a metal cylinder carries a wire gauze funnel which leads laterally to a small chamber fitted with a collecting bottle; the bottle contains a killing agent—potassium cyanide or pyrethrum.

The switching of the fan motor is performed by a 'Sunvic' hot-wire vacuum switch, using as a control an ordinary alarm clock the minute hand of which trails a piece of hair-spring over a surface of aluminium foil out of which are cut sectorial pieces to correspond with the desired 'off' periods.

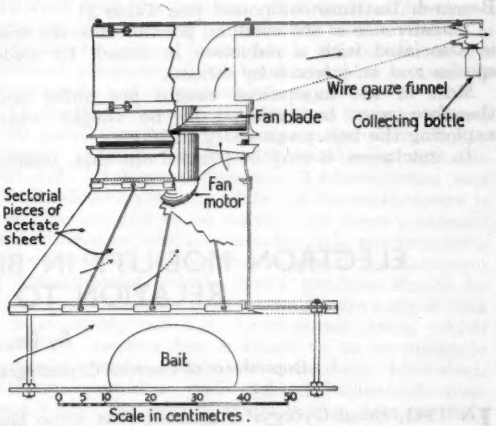

Fig. 1

Comparisons of the performance of the first design of trap on various cycles of operation—continuous running and 'on' and 'off' periods, respectively, and 2 and 3 min., 5 and 5 min., and 2 and 8 min.—led to the selection of 'on' 2½ and 'off' 5 min. periods giving eight complete cycles per hour. This cycle has been retained in the larger trap with, so far, no further experimentation. Other pilot studies with the first design of trap concerned the effect of different animal baits, the periodicity of activity and the effects of different environments and levels. All these studies gave promising results although the numbers caught were only moderate; for example, in sixteen nights at ground-level in the compound of the Institute, 940 mosquitoes were taken on four guinea pigs as bait—an average of 59 per night. The new design appears much more effective; under approximately similar conditions both with regard to trap operation and expected mosquito density the new design took 2,449 mosquitoes on one rabbit in five nights, an average of 490 mosquitoes per night.

Some first results to indicate the efficiency of the new design are as follows:

(*a*) The effect of the bait; 10 all-night runs, alternately with and without a grey rabbit; ground-level, East African Virus Research Institute compound (see Table 1).

The occurrence of small numbers of mosquitoes in the trap in the absence of bait is to be expected as the trap will catch mosquitoes entering it by chance at the appropriate time. However, in this particular case a cage which, fouled by the animal, may have some attractive power, was present in both series.

Table 1

Mosquito species	Arithmetic mean number of females caught per night	
	With rabbit	Without rabbit
Taeniorhynchus metallicus	1·0	0·6
fuscopennatus	475·8	57·6
africanus	9·0	2·0
uniformis	1·4	0·0
Culex spp. indet.	2·6	0·0
Total—mean	2,449—489·8	301—60·2

(*b*) The effect of position; 12 all-night runs, alternately at ground-level and 3 m. above it, in a tree; bait, one grey rabbit; East African Virus Research Institute compound (see Table 2).

Transference of the catching position into the tree is associated with a reduction in attack by some species and an increase by others.

Most of the mosquitoes caught are unfed and therefore may be assumed to be caught when exploring the bait preparatory to biting.

In conclusion, it may be pointed out that, besides the uses indicated above, the trap has possibilities for the investigation of the potency of attractants other than those emitted by the bodies of animals.

Table 2

Mosquito species	Arithmetic mean number of females caught per night	
	Ground	Tree
Uranotaenia sp.	0·0	0·2
Ficalbia plumosa	0·2	0·0
Taeniorhynchus metallicus	0·5	3·2
maculipennis	0·0	0·2
fuscopennatus	269·5	26·7
aurites gp.	0·7	4·7
africanus	10·8	0·0
uniformis	0·8	0·0
Aëdes africanus	0·2	0·2
Culex annulioris	0·0	0·2
spp. indet.	1·3	11·8
Total—mean	1,704—284·0	282—47·0

[1] Haddow, A. J., *Bull. Ent. Res.*, **45**, 199 (1954).
[2] Magoon, E. H., *Bull. Ent. Res.*, **26**, 363 (1935).
[3] Bellamy, R. E., and Reeves, W. C., *Mosquito News*, **12**, 256 (1952).
[4] Lumsden, W. H. R., E.A.V.R.I. Ann. Rep., No. 6 (Governmen Printer, Nairobi, 1956).

ELECTRON MOBILITY IN BIOLOGICAL SYSTEMS AND ITS RELATION TO CARCINOGENESIS

By Dr. R. MASON

Department of Chemical Crystallography, University College, London, W.C.1

In 1941, Szent-Györgyi[1] suggested that some biochemical processes could be better understood if protein molecules and extended systems of proteins were imagined to possess common energy bands similar to those which are derived from the collective-electron model of crystals; the conductivities of dyed protein films were later cited[2] as evidence for the usefulness of such an approach, although such limited data were scarcely conclusive. A good deal of other suggestive, but still not conclusive, evidence led Evans and Gergely[3] to examine theoretically the possibility of bands of energy-levels in proteins as a consequence of electronic conjugation through the π-orbitals in a protein structure represented by the scheme shown in Fig. 1.

These authors concluded that a banded structure was possible and indeed would actually confer a small extra stability on the system; their results, with nitrogen assumed to be trigonal planar (the most likely stereochemistry), are given in Table 1, which lists the range of energy bands in eV. relative

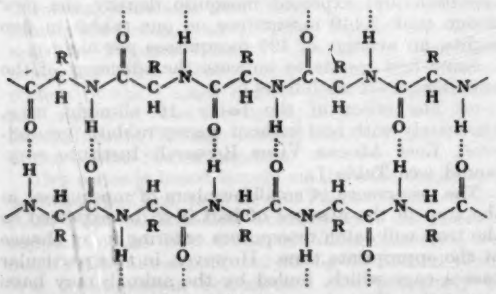

Fig. 1

Table 1. Range of Energy Bands (eV.)

Band 1	0·00–0·13	Doubly filled
Band 2	3·17–3·43	Doubly filled
Band 3	6·48–6·60	Unfilled

to the lowest filled level, together with the electronic population of the bands.

Three main conclusions were discussed by Evans and Gergely: a protein structure is non-conducting in the ground-state and can assume conducting properties only on excitation; the width of the forbidden zone (~ 3 eV.) is such that conduction can never be realized by thermal excitation, but can take place by the absorption of photons of suitable energy; finally, the narrowness of the bands, as compared with a metal or graphite structure, suggests "that in order to transfer electrons to or from empty or full levels of another system, the energy levels of the structures must be closely matched".

The calculations received, perhaps unexpectedly, excellent confirmation from the measurements of Eley *et al.*[4] of the semiconductivity of proteins. Plasma albumin, fibrinogen and edestin were shown to possess conducting properties which are consistent with a band structure having an energy gap of 2·1–2·6 eV. Later work[5] described dry hæmoglobin as having a semiconductivity with an energy gap of 2·7 eV., while separation into globin and hæm gave observed gaps of 3·0 and 1·8 eV., respectively; it was also suggested that while these experiments had been carried out on denatured proteins, similar results were indicated for natural hæmoglobin.

The importance of the conclusion reached by Evans and Gergely regarding the requirement for close matching of energy-levels if charge transfer is to

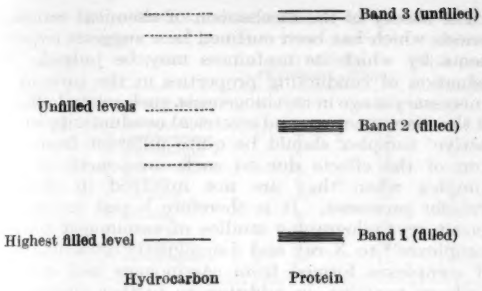

Fig. 2

take place can be discussed from the point of view of a molecular complex formed from a protein and an aromatic hydrocarbon (the latter is chosen because of the availability of data from quantum-mechanical methods concerning energy-levels, etc.). The conditions which must be satisfied in such a complex, if electrons are to be transferred from one molecule to the other, have been pointed out by various authors (see, for example, ref. 6), and may be written in the form

$$h\nu = I_D - E_A - \Delta$$

where $h\nu$ is the energy required to bring about charge transfer, I_D the ionization potential of the donor, E_A the electron affinity of the acceptor and Δ the stabilization of the ion pair. Let us assume that the electronic condition of the complex before any charge transfer takes place can be represented as in Fig. 2, that is, the highest filled level of the hydrocarbon coincides with the lowest filled band of the protein. This assumption, implying that

$$I_D \text{ (band 1)} = E_A + \Delta$$

cannot, unfortunately, be examined in any rigorous fashion so far as we know and its only justification is the results which follow its adoption. Our object—to alter the electron population of the highest filled band in the protein in such a way that conduction can take place—will now be achieved when an unfilled level of the hydrocarbon occurs with an energy which is within the energy range of the highest filled band in the protein, that is, when the energy separation between the highest filled and the nth unfilled level of the hydrocarbon

$$\Delta E_n = 3{\cdot}23 \pm 0{\cdot}19 \text{ eV.}$$

If this condition is fulfilled, the hydrocarbon may be regarded as 'active', and this activity, which therefore corresponds to the induction of electron mobility in the protein, will afterwards be considered to be an essential prerequisite in the process of carcinogenesis by these molecules.

Table 2 gives the energy difference between the highest filled and successive unfilled levels, in units of β (the resonance integral), as calculated by the simple molecular orbital method; these values have been calculated from the "Dictionary of Values of Molecular Constants"[7], and Prof. B. Pullman has generously provided energy-level schemes for a number of molecules which do not appear in those tables. The carcinogenic indices are taken, in most part, from Hartwell's[8] and Pullman's[9] surveys. 'Activity' can be seen to correspond approximately to the range

$$1{\cdot}10 \leqslant \Delta E_n \leqslant 1{\cdot}18$$

with the unit of energy on this scale, therefore, approximately 2·8 eV., a value in not unreasonable agreement with spectroscopic data. The relation suggests that the matching of energy-levels may be even more critical than is indicated by Evans and Gergely's calculated band-widths, but this must not, perhaps, be considered too seriously when the shortcomings of the theoretical method are remembered. It can also be seen from Table 2 that the molecules which satisfy this energy criterion possess, with the exception of 3,4-benzophenanthrene, one or more unfilled levels which lie below those coming into coincidence with the highest filled band in the protein; once electron mobility has been induced in the protein, these lower unfilled levels of the hydrocarbon must be imagined to act as further 'sinks' for electrons from the protein. The result of forming a complex of an 'active' hydrocarbon and a protein is therefore to induce positive-hole conduction in the hitherto completely-filled band and also to transfer electrons to a state of lower energy.

Of the 34 hydrocarbons tabulated, five are known, with some certainty, to be carcinogens of varying potency, namely, 3,4-benzophenanthrene, 1,2 : 5,6- and 1,2 : 7,8-dibenzanthracene, 3,4-benzpyrene and 3,4 : 8,9-dibenzopyrene, while 1,2-benzanthracene is now also thought to be active. All these molecules satisfy the stipulated criterion for ΔE_n, the remaining molecules falling outside the range with the exception of anthanthrene, which theory predicts should be active (very limited experimental results suggest that it is inactive), and 1,2 : 7,8-dibenzofluorene, which should be inactive but is found to be carcinogenic (although there appears to be some doubt here also). 1–12-benzoperylene and 2,3 : 6,7-dibenzofluorene come within the 'active' range but no data on their physiopathological properties are available.

Several molecules appear in Table 2 in which intramolecular overcrowding is known to exist; the

Table 2

Molecule	ΔE_1	ΔE_2	ΔE_3	ΔE_4	Activity
Benzene	2·0000	2·0000	3·0000		−
Naphthalene	1·2361	1·6180	1·9208	2·2361	−
Anthracene	0·8284	1·4142	1·4142	1·8284	−
Naphthacene	0·5899	1·0724	1·2950	1·4885	−
Pentacene	0·4394	0·8377	1·2197	1·2197	−
Phenanthrene	1·2105	1·3743	1·7476	1·9110	−
3,4-Benzophenanthrene	1·1352	1·2297	1·5676	1·6910	+
Chrysene	1·0402	1·3124	1·3954	1·7365	−
1,2-Benzanthracene	0·9046	1·1673	1·4523	1·6179	+ ?
Pyrene	0·8901	1·3244	1·4450	1·6920	−
Triphenylene	1·3681	1·5634	1·9696	2·0313	−
Pentaphene	0·8743	0·9580	1·4372	1·4372	−
Picene	1·0038	1·1822	1·3614	1·5019	−
Perylene	0·6946	1·3473	1·3473	1·3473	−
1,2 : 3,4-Dibenzanthracene	0·9982	1·2131	1·2899	1·5940	−
1,2 : 5,6-Dibenzanthracene	0·9470	1·1578	1·2601	1·5431	+ +
1,2 : 7,8-Dibenzanthracene	0·9834	1·1097	1·3652	1·4917	+
3,4 : 5,6-Dibenzophenanthrene	1·0709	1·1922	1·3227	1·6027	−
3,4-Benzpyrene	0·7425	1·1730	1·3712	1·3712	+ + + +
3,4 : 8,9-Dibenzopyrene	0·6054	1·0960	1·1314	1·3027	+ + + +
1–12-Benzoperylene	0·8784	1·1235	1·4329	1·4392	?
Coronene	1·0784	1·0784	1·5392	1·5392	−
Anthanthrene	0·5818	1·0409	1·1657	1·3944	−
Diphenyl	1·4092	1·7046	1·7046	2·0221	−
p-Diphenylbenzene	1·1853	1·5926	1·5926	1·5926	−
m-Diphenylbenzene	1·3243	1·4275	1·6622	1·6622	−
o-Diphenylbenzene	1·2206	1·4346	1·6103	1·6103	−
1,3,5-Triphenylbenzene	1·3243	1·5681	1·6622	1·6622	−
Styrene	1·3242	1·6622	2·0764	2·7979	−
Stilbene	1·0086	1·5043	1·5043	1·6577	−
Biphenylene	0·8901	1·3244	1·6920	1·7923	?
2,3 : 6,7-Dibenzofluorene	0·9687	1·0107	1·1518	1·6211	?
3,4 : 5,6-Dibenzofluorene	0·7466	1·2002	1·2151	1·4433	−
1,2 : 7,8-Dibenzofluorene	0·8326	1·1848	1·3793	1·5207	+ ?

calculations of the energy-levels for these compounds can scarcely have any reliability placed upon them and this may account for the exception which 3,4-benzophenanthrene makes to the other carcinogens in not having an unfilled level below that level which coincides with the highest filled band in the protein.

When electrons from the highest filled band fall to lower unfilled levels in the hydrocarbon component of the complex, work (possibly in the form of some synthesis) may be done on the system, and Szent-Györgi's suggestions as to "why catabolic processes prevail over anabolic ones in damaged tissues (and cancer ?) . . ." are, perhaps, now more readily understood for the particular example of chemical carcinogenesis. It is therefore tempting to suggest that the relative potency of the carcinogen is related to the energy gap through which electrons from the highest filled band fall to reach unfilled levels in the hydrocarbon (and the data of Table 2 suggest that this may not be a completely misleading concept), but here again we must recognize the inadequacies of the theoretical methods and the present paucity of data.

The banded electronic model of a protein suggests, also, that radiation carcinogenesis may be interpreted as the result of electron promotion from the highest filled band to the conduction band proper of the protein. It follows that the overall result is one of inducing positive-hole conduction in the highest filled band and afterwards performing work on the system when the electrons promoted by photon absorption fall back to their ground state. If oxygen adsorption leads to impurity-levels close to the filled band, electron promotion will be facilitated and we may thus be able to understand the increase in radiation sensitivity, which malignant cells show on adsorption of oxygen, in this way.

The theory of the mechanism of chemical carcinogenesis which has been outlined here suggests experiments by which its usefulness may be judged. If induction of conducting properties in the protein is a necessary stage in carcinogenesis, such related effects as the diamagnetism and electrical conductivity of an 'active' complex should be quite different from the sum of the effects due to each component of the complex when they are not involved in charge-transfer processes. It is therefore hoped to extend recent crystallographic studies of carcinogen–purine complexes[10] to X-ray and diamagnetic investigations of complexes formed from carcinogens and nucleic acids or proteins, in addition to further theoretical consideration of these molecules.

Finally, it should be pointed out that the electronic structure of a protein ion in an active complex will be quite different from that usually assumed for protein ions in solution. In the latter case, the net charge is due to ionization in the side-chains of the protein, and it is unlikely that such perturbations can conjugate through these chains to the electronic system in which we have been interested.

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CONTRIBUTION OF DEOXYMETHYLPENTOSES TO THE SEROLOGICAL SPECIFICITY OF SOME BACTERIAL POLYSACCHARIDES, AND THE RECOGNITION OF A NEW SUGAR, PARATOSE

By Dr. D. A. L. DAVIES
Microbiological Research Establishment, Porton, Wilts

Dr A. M. STAUB
Institut Pasteur, Paris

AND

Dr. I. FROMME, Dr. O. LÜDERITZ and Prof. O. WESTPHAL
Dr. A. Wander Forschungsinstitut, Freiburg-Zähringen

TWO different deoxymethylpentoses have been found in specific polysaccharides extracted from various species of *Salmonella*, tyvelose in the products of group *D* strains (*S. typhosa*[1–3] and *S. enteritidis*[2]) and abequose in the products of group *B* strains (*S. abortus-equi*, *S. paratyphi B*[2–4] and *S. typhimurium*[4]). These sugars are very rapidly liberated from the polysaccharides on acid hydrolysis, which suggested that they might occupy terminal positions in the polymer chains and contribute to the specificity of groups *D* and *B*. Analyses of polysaccharides extracted from other species of *Salmonella* confirmed the presence of tyvelose in other group *D* strains and of abequose in other group *B* strains[5] (Table 1), but the presence of sugars having similar chromatographic behaviour to tyvelose and abequose was revealed in members of other groups (*S. paratyphi A* in group *A*; *S. Kottbus* and *S. newport* in group C_2)[5].

It has been shown that specific reactions of immune sera with homologous polysaccharides can be inhibited by single sugars[6]. In the *Salmonellae*, abequose inhibits the precipitation of an anti-*S. paratyphi B* serum by the homologous polysaccharide and tyvelose inhibits precipitation of an anti-*S. typhosa* serum by the homologous polysaccharide. Neither abequose nor ascarylose inhibits the latter system[7] although ascarylose, a deoxymethylpentose extracted from

Table 1. Composition of the Specific Polysaccharides of Groups *A*, *B* and *D* *Salmonellae*

Group	Species	'O' antigen (Kauffman–White)				Galactose	Glucose	Mannose	Rhamnose	Abequose	Paratose	Tyvelose
A	*S. paratyphi A*	1	2		12	+	+	+	+		+	
B	*S. paratyphi B*	1	4	5	12	+	+	+	+	+		
	S. typhimurium	1	4	5	12	+	+	+	+	+		
	S. abortus-equi		4		12	+	+	+	+	+		
	S. budapest	1	4		12	+	+	+	+	+		
	S. stanley		4	5	12	+	+	+	+	+		
	S. salinatis		4		12	+	+	+	+	+		
D	*S. typhosa*		9		12	+	+	+	+			+
	S. enteritidis	1	9		12	+	+	+	+			+
	S. gallinarum	1	9		12	+	+	+	+			+
	S. dar-es-salaam	1	9		12	+	+	+	+			+

Parascaris equorum, cannot be distinguished chromatographically from tyvelose[6]. These results indicated that tyvelose and abequose do contribute to the specificity of groups *D* and *B* *Salmonellae* and suggested that the specificities on which the Kauffman–White scheme is based are related, at least in part, to their content of the respective deoxymethylpentose. It could also be visualized that sugars present in the polysaccharides of *Salmonellae* of groups other than *D* and *B*, which could not be distinguished chromatographically from tyvelose and abequose, might in fact be isomers with closely similar properties. A re-examination of the composition of *S. paratyphi A* specific polysaccharide, previously reported to contain tyvelose, is recorded here, and the presence of a deoxymethylpentose has been established which is not tyvelose, nor is it identical with any sugar of this series previously recorded.

Cells of *S. paratyphi A* were extracted by the phenol/water method and the lipopolysaccharide component recovered in purified form by high-speed centrifugation[3]. After hydrolysis with *N* sulphuric acid, analysis on paper chromatograms, using authentic sugar samples for comparison, revealed the following sugars: galactose, glucose, mannose, rhamnose and a sugar which migrated faster than rhamnose and had an R_F value close to that of tyvelose. When hydrolysates of lipopolysaccharides obtained from *S. paratyphi A*, *S. typhosa* and *S. abortus-equi* were examined chromatographically side by side it became clear that the fast-running component of *S. paratyphi A* ('Paratose') was not identical either with tyvelose or abequose; the $R_{rham.}$ values were abequose 1·16, tyvelose 1·29 and paratose 1·25 (pyridine–butanol–water, 4 : 6 : 3). Like tyvelose and abequose, paratose is the first sugar to be released from its polysaccharide on acid hydrolysis. A sample of paratose was isolated by elution from paper chromatograms; it contains one oxidizable aldehyde group determinable quantitatively by the Willstätter–Schudel method. Oxidation with 1 mole of periodate produced, besides formic acid, a breakdown product which gave a strongly positive Webb reaction[9]; this is an intense red colour which develops on heating with *p*-nitrophenylhydrazine in trichloracetic acid, followed by the addition of strong alkali. The reaction is characteristic of 2-deoxysugars which have at least a C_5 straight chain; 2-deoxytetroses do not react[10]. A strongly positive reaction has also been obtained with the C_6 periodate breakdown products of tyvelose and abequose. It therefore seems probable that, like tyvelose and abequose, paratose belongs to the group of naturally occurring deoxymethylpentose sugars, recently characterized fully as 3.6-*bis*deoxy-aldohexoses[11].

From Table 1 it will be seen that galactose, glucose, mannose and rhamnose are present in all the *Salmonella* polysaccharides investigated, which include representatives of groups *A*, *B* and *D*. Group specificity, however, is clearly associated with the presence of paratose, abequose and tyvelose.

Although deoxymethylpentoses have been isolated thus far only from one group of bacteria (*Salmonellae*)[2,3] and from one animal source[8], it is clear that they are more widely distributed, at least among bacteria. In the 'O' antigens of some *Escherichia coli* strains (O 55 and O 111) deoxymethylpentoses have been found[3] which behave chromatographically like abequose; studies of their chemical identity and immunological role are in progress. In the *Pasteurellae*, smooth forms of *P. pseudotuberculosis* yield lipopolysaccharides containing a number of different sugars[12], and from each of the five serological groups[13] the lipopolysaccharidé possesses a different specificity. In two instances (groups IV and V), one component could not be distinguished chromatographically from tyvelose; it is noteworthy that a serological cross-reaction has been recorded between *P. pseudotuberculosis* and *Salmonellae* of group *D* [14,15], the latter containing tyvelose. Hydrolysates of *P. pseudotuberculosis* group II lipopolysaccharide revealed on chromatographic analysis a sugar indistinguishable from abequose. Erythrocytes sensitized with the group II lipopolysaccharide can be agglutinated by *Salmonella* group *B* antisera, and erythrocytes sensitized with *Salmonella* group *B* lipopolysaccharide (for example, *S. salinatis*), which contain abequose, are agglutinated by *P. pseudotuberculosis* group II antisera.

In the remaining *P. pseudotuberculosis* groups (I and III) not all the sugars have been identified but fast running components appear to be present. However, in all instances isolation of the sugars will be necessary for a satisfactory identification.

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HAPTOGLOBIN TYPES IN BRITISH, SPANISH BASQUE AND NIGERIAN AFRICAN POPULATIONS

By Dr. A. C. ALLISON*, Dr. B. S. BLUMBERG† and Mrs. ap REES
Department of Biochemistry, University of Oxford

SMITHIES[1,2] found that the patterns obtained by starch gel electrophoresis of sera from different human subjects fell into three groups. The determinant proteins combine with hæmoglobin and appear to be identical with the haptoglobins previously studied by Jayle and his colleagues[3,4]. It has therefore been suggested by Smithies and Walker[5] that the groups should be named haptoglobin types 1–1, 1–2 and 2–2 (formerly types I, II*A* and II*B*, respectively). Preliminary genetical analysis[6] indicated that the groups correspond to the three genotypes produced by a single pair of allelomorphic genes. In fact, our unpublished observations suggest that the genetical control of the haptoglobin types is more complex. A detailed discussion of the evidence will shortly be submitted for publication. In the meantime results can be expressed in the nomenclature of Smithies and Walker, with the addition of a fourth group of subjects in which no haptoglobin at all can be detected, for which the designation type O–O is suggested.

The results of our three surveys, together with those of Smithies on Canadians and Sutton *et al.* on Americans and Africans from Liberia and the Ivory Coast, are summarized in Table 1. The frequencies of the various haptoglobin types in the British series are not significantly different from those in the small Canadian and American series, although the former includes six subjects of type O–O, none of which was described by Smithies or by Sutton *et al.* The frequencies in the Basque series also are not significantly different from those in the British, Canadian and American series. Thus the uniqueness of the Basques, shown in their low *B* and *Fy*[a] and high *Rh*-negative frequencies[8,10], is not reflected in their haptoglobin types.

Table 1. Haptoglobin Types in Various Populations

Population	No. tested	0—0		1—1		1—2		2—2		Authors
		No.	(Per cent)	No.	(Per cent)	No.	(Per cent)	No.	(Per cent)	
Canadian	49	0	0	10	21·1	25	50·5	14	28·4	Smithies
American	54	0	0	6	11·1	29	53·7	19	35·2	Sutton *et al.*
British	218	6	2·7	22	10·1	121	55·5	69	31·7	Allison *et al.*
Basque	107	1	0·9	15	14·0	49	45·7	42	39·3	Allison *et al.*
Liberian and Ivory Coast (African)	142	0	0	69	48·7	60	42·3	13	9·1	Sutton *et al.*
Nigerian (African)	99	32	32·3	53	53·5	11	11·1	3	3·0	Allison *et al.*

The frequency of the haptoglobin types in different populations is of anthropological and genetical interest. It may also provide a clue to the way in which this remarkable polymorphism is maintained. The only published population studies of which we are aware are those of Smithies[2] on Canadians and of Sutton *et al.*[7] on Americans and on West Africans from Liberia and the Ivory Coast. In this article the results of haptoglobin type analyses of 218 British subjects, 107 Spanish Basques and 99 Nigerian Africans are presented.

The British sera were taken from unselected laboratory staff and blood donors in the Oxford region. The Basque bloods were collected as described elsewhere[8]. The African sera were taken from Yoruba of Ilobi, Western Nigeria.

Fresh oxyhæmoglobin was added to all sera to give a final concentration of the order of 100–120 mgm. per 100 ml. before electrophoresis in starch gels[2]. Each gel was then cut horizontally and one half stained with benzidine, as described by Allison and ap Rees[9]. The addition of enough hæmoglobin nearly to saturate the haptoglobins, but not much more nor less, and benzidine staining of the gels after electrophoresis, greatly facilitates the identification of the haptoglobin types, which may not otherwise be easy. As will be mentioned below, the discrepancy between our results and those of Sutton *et al.* on West Africans may be attributable to differences in technique.

The high frequency of Nigerians without haptoglobins raises a physiological point. It has been shown that the 'renal threshold' for hæmoglobin is due to binding of the pigment by haptoglobins in the serum[9,11]. In subjects without haptoglobins most hæmoglobin liberated into the blood stream passes into the urine. Since hæmoglobinæmia must frequently occur after malarial attacks as well as after vigorous exercise[9], it might be expected that some degree of hæmoglobinuria and loss of iron

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* External member of the scientific staff of the Medical Research Council. Present address: National Institute for Medical Research, London, N.W.7.

† Fellow of the Arthritis and Rheumatism Foundation.

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in the urine would be common among Africans. To our knowledge, this point has not been investigated.

The fact that several haptoglobin types exist side by side in a number of different populations indicates that it is a true polymorphism. This may have been maintained, like those of the blood groups and abnormal hæmoglobins, by a balance of selective forces related to susceptibility to disease[12]. The absence of haptoglobins might be disadvantageous, particularly in regions where there is a deficiency of iron in the diet. The fact that so many Nigerians lack haptoglobins is presumably due to a compensatory advantage in persons heterozygous for the factor concerned. The low frequency of type 2–2 in the tropics suggests that it may confer protection against one or more conditions common in temperate, but not in tropical, climates.

Dr. F. Alberdi kindly co-operated in the collection of the Basque blood specimens and Drs. Jean Grant and H. Preston in the collection of British bloods. The Nigerian specimens were collected by one of us (B. S. B.) as part of the work carried out on an expedition supported by the Wenner Gren Foundation, Inc., and grant No. *H*-3161 from the National Heart Institute, U.S. Public Health Service. The help of Prof. B. G. T. Elmes, Ibadan, is gratefully acknowledged.

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ONE-ELECTRON *VERSUS* TWO-ELECTRON TRANSFER PROCESSES IN THE MECHANISM OF OXIDATION-REDUCTION REACTIONS IN SOLUTION

By Prof. JOSEPH WEISS
University of Durham, King's College, Newcastle upon Tyne

In recent years there has been some discussion regarding the question of one-electron *versus* two-electron transfer processes in the mechanism of oxidation-reduction processes in solution. The concept of one-electron transfer processes, in its general form, was first put forward by Haber[1], and it has been confirmed by a large body of experimental evidence. The assumption of single electron transfer also leads directly to free radical intermediates which play such an important part in the mechanism of chemical reactions.

One-electron transfer processes also constitute an essential step in many photochemical (ref. 2*a*) and photosensitized reactions (ref. 2*b*) in solution. Moreover, it has been suggested that the formation of many of the so-called 'molecular complexes' is also due essentially to an electron transfer between the two components (ref. 2*c*).

In general, it is to be expected that one-electron transfer processes should predominate over two-electron transfers, as the latter are only likely to occur when there is a strong coupling between the electrons.

Although it is now generally accepted that in the majority of cases one deals with one-electron transfer processes, it is believed that there are certain oxidation reactions where two-electron transfer predominates[3]. In the latter cases, this assumption is mainly derived from the apparent inability to detect free radicals, and from the appearance of oxygen from the oxidizing reagent in the molecule which has been oxidized (as demonstrated by means of compounds labelled with oxygen-18).

In the following, an attempt has been made to show that all the relevant experimental facts are fully compatible with, and can be easily explained by, one-electron transfer processes.

From the theory of electron-transfer processes it follows (ref. 4*a*, *b*) that one of the significant features is that electron transfer leads to a more or less strongly bound primary (ionic) complex between the reacting partners; thus, in the case of two uncharged reactants: $A + B \rightarrow (A^{+}.B^{-})$. In general, the energy of interaction gained in the formation of the primary complex also makes an important contribution towards the 'driving force' of the electron transfer. The ionic complexes primarily formed will have a certain minimum life-time. An estimate of the probable life-time of such a complex in solution can be obtained from the time which is required for the water dipoles to interact with an ionic complex; this cannot be less than the time of relaxation of water dipoles in liquid water, which is of the order of 10^{-10}–10^{-11} sec. This will, however, only be a lower limit, because in the interaction of water molecules with an ion, the time of relaxation in the immediate neighbourhood of the ion will have to be considered. According to the work of Kozyrev[5] and others, these relaxation times can be considerably greater than those measured for the pure solvent and in certain cases they can be as long as 10^{-7} sec.

This may have the consequences, first, that the primary complex could be long lived enough to react as a separate entity with another reactant present in the solution, and secondly that the possibility of intramolecular changes within the complex must be taken into account. This follows from the consideration that the jump frequency for atoms (or ions) is given by $\nu = \nu_0 \exp(-E/kT)$, where E is the activation energy and ν_0 (frequency coefficient) is of the order of 10^{13}–10^{14} sec.$^{-1}$ for a typical atom (or ion) transfer. Moreover, such changes should have a negligible or only a very small energy of activation, as both the reacting species have free-radical character.

Thus, for example, even in a primary complex of the relatively short life-time of 10^{-10} sec., about $10^{-10} \times 10^{14} = 10^4$ jumps can take place if the process goes without appreciable energy of activation, and even up to activation energies of about 4 kcal. there would still be a high probability for an intramolecular transfer of a heavy particle; in the case of longer lived intermediates the situation is, of course, more favourable.

There is, therefore, a considerable probability that, during the life-time of the primary complex, intramolecular transfer of atoms or radicals will take place without any appreciable participation of the solvent.

A case where the primary complex is probably long lived enough to react with another molecule is found in the oxidation of ferrous ions by oxygen in acid solution[6], where presumably the primarily formed electron transfer complex, namely, $Fe^{3+} + O_2 \rightarrow (Fe^{3+} . O_2^-)$, reacts with another ferrous ion giving $[Fe^{3+} . O_2^- . Fe^{2+}]$, and this dinuclear complex then breaks up to give ferric ions and hydrogen peroxide. Additional support for this type of intermediate comes from the autoxidation of cobaltous amine ions where complexes of the structure $[Co^{3+}(NH_3)_5 . O_2^{2-} . Co^{3+}(NH_3)_5]^{4+}$ have actually been isolated.

Examples of supposedly two-electron transfer processes which can be explained on the basis of one-electron transfers are the following.

The reaction between hydrogen peroxide and iodide ions (ref. 7*a*). The first step is expected to be electron transfer according to the reaction

$$I^- + H_2O_2 \rightarrow (I . H_2O_2^-) \qquad (1a)$$

followed by association of the iodine atom and OH radical within the primary complex:

$$(I . H_2O_2^-) \rightarrow I . OH + OH^- \qquad (1b)$$

Oxidation of sulphite by chlorate. From the kinetics of this reaction it follows that the first step is a reaction between chlorate ion and sulphurous acid, and it has been shown by means of chlorate labelled with oxygen-18 that transfer of (about 2·3) oxygen atoms from the chlorate to the sulphite takes place (ref. 7*b*). On the basis of the theory given above, this reaction can be represented as follows:

$$ClO_3^- + SO_2 \rightarrow (ClO_3^{2-} . SO_2^+) \qquad (2a)$$

$$(ClO_3^{2-} . SO_2^+) \rightarrow (ClO_2^- . SO_3) \qquad (2b)$$

$$(ClO_2^- . SO_3) \xrightarrow{H_2O} ClO_2^- + H_2SO_4 \qquad (2c)$$

with the corresponding reactions between the intermediately formed ClO_2^- and ClO^- (or HClO) with SO_2. Thus, one has single electron transfer processes followed by O^- transfers within the complex, possibly with some solvent interaction in the last stage.

Oxidation of sulphite by hydrogen peroxide (ref. 7*b*). This is a particularly interesting case because one now has an explanation of a hitherto unexplained fact that, whereas in the oxidation of sulphurous acid in acid solution two oxygen atoms from the hydrogen peroxide are transferred, in alkaline solutions, only one of the oxygen atoms of the hydrogen peroxide appears in the resulting sulphate[8]. According to the theory outlined here the reactions can be formulated as:

In acid solution:

$$H_2O_2 + SO_2 \rightarrow (H_2O_2^- . SO_2^+) \qquad (3a)$$

$$(H_2O_2^- . SO_2^+) \rightarrow (HO . SO_3H) \qquad (3b)$$

At higher *p*H:

$$H_2O_2 + HSO_3^- \rightarrow (H_2O_2^- . HSO_3) \qquad (3c)$$

$$(H_2O_2^- . HSO_3) \rightarrow HO.SO_3H + OH^- \qquad (3d)$$

Thus, in acid solutions, an OH^- and an OH radical may be transferred within the primary ionic complex, whereas the radical ions (HSO_3 or SO_3^-) formed from the sulphite anions should only associate with the OH radical but not with the OH^-.

Biological oxidation-reduction processes. The interesting results of Vennesland and Westheimer[9] concerning the reaction between deuterated ethanol ($CH_3.CD_2OH$) and diphosphopyridine nucleotide, catalysed by alcohol dehydrogenase, are fully in agreement with the general theory given above. (Proviso must be made here, of course, with regard to any possible role of the enzyme in the actual reaction mechanism which is, as yet, unknown.) If the reaction is assumed to take place directly between the two reactants, it could be represented by a one-electron transfer followed by a deuterium (or hydrogen) atom transfer, within the primary complex, from the alcohol radical ion to the monovalently reduced nucleotide. In the cases of hydrogen or deuterium atom transfer, there would also be an increased probability of transfer by a tunnel effect. As all such processes would take place during the life-time of the primary complex, it is not at all surprising that they proceed without hydrogen exchange with the solvent, as found experimentally.

Formation of free radicals. It is known that in the majority of oxidation-reduction processes free radicals do, in fact, appear as more or less stable intermediates. The occurrence of these can often be predicted on plausible physical grounds. For example, in the reaction (ref. 1*b*)

$$Fe^{2+} + H_2O_2 \rightarrow (Fe^{3+} . H_2O_2^-)$$

the primary complex would be expected to break up by interaction with the solvent according to the equation

$$(Fe^{3+} . H_2O_2^-) \rightarrow Fe^{3+}OH^- . aq + OH$$

giving OH radicals. Similarly, in the reaction between oxalate ions ($C_2O_4^{2-}$) and iodine atoms, the primary complex should break up to give the radical ion $C_2O_4^-$ thus

$$(C_2O_4^- . I^-) \rightarrow C_2O_4^- . aq + I^-$$

One may conclude, therefore, that in general the appearance of a free radical as a reacting entity depends on the intrinsic behaviour of the primarily formed one-electron transfer complex. This complex may release into the medium either a free radical or a valence-saturated species[10], the latter arising from a rearrangement within the complex prior to its break-up by the solvent (water).

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INDUCED GENE UNSTABILIZATION

By Prof. STEPHEN ZAMENHOF, Miss ROSALIE de GIOVANNI and Dr. SHELDON GREER
Department of Biochemistry, College of Physicians and Surgeons, Columbia University, New York 32

A GENE may acquire a hereditary high mutability-rate either by its own mutation to an unstable form or as a result of the influence of another gene (which in turn is changed by a mutation or is introduced *de novo* into the genome). Whatever the cause, the resulting gene unstabilization might be of importance for evolution.

In this report, the observable phenomenon of unstability rather than the precise mechanism will be discussed. The experiments were performed on strains of *Escherichia coli* which have not been reported to recombine sexually; therefore, it is assumed that in this case the unstabilization is caused solely by a mutation (and not *de novo* introduction of another gene). In such strains, the site of the mutation (nuclear or cytoplasmic) cannot be unequivocally established; however, in the cases of induced unstabilization studied here, the effects were a result of the known interference with the structure of deoxyribonucleic acid molecules, and this points to this acid as the site of these changes.

The 'unstable gene' will be arbitrarily designated as the one with a spontaneous mutation-rate higher than 1 per 1,000 cells per generation. Studies of such high mutabilities were avoided in the past, partially because of the lack of proper techniques.

The cells of *Escherichia coli* can replace up to half the thymine in their deoxyribonucleic acid[1–3] by 5-bromouracil without losing the ability for subsequent division[4]. This replacement is reversible[5]. It occurred to us[4] that such a drastic (though temporary) change in the structure of deoxyribonucleic acid, which is believed to be the hereditary determinant[6], may result in a change in genotype: the recent findings that the replacement can occur without apparent cell division[3,5] and that the precursor is a nucleotide[7] (which may imply breakages in the sugar–phosphate chain of deoxyribonucleic acid) suggest the possibilities of faults during the replication of deoxyribonucleic acid even though 5-bromouracil afterwards disappears from the acid upon cultivation on normal medium.

We have reported that the spontaneous mutation-rate of strain *I* to the 5-bromouracil resistant strain *I/Br* was not changed by the introduction (and subsequent removal) of 5-bromouracil into and from deoxyribonucleic acid[4]. We now find that the spontaneous and ultra-violet-induced mutation-rates (both to prototrophy and to auxotrophy) of eight different markers in four different strains, tested in various media, with various concentrations of 5-bromouracil, and with and without intermediate cultivation, were also not changed. (Purine − mutant of strain *W*: mutation to purine +. Strain *I*: mutation to thymine +, streptomycin resistance, total auxotroph count. Strain A.T.C.C. 11887: mutation to lactose −. Uracil − mutant of strain A.T.C.C. 11887: mutation to uracil +, lactose +, total auxotroph count.) It must be concluded that the introduction (and subsequent removal) of 5-bromouracil to (and from) deoxyribonucleic acid is not a process that unstabilizes all the genes. It was of interest to study whether some genes can be unstabilized by such a process.

In a previous publication[4] we have reported that during the experiments on introduction of 5-bromouracil into deoxyribonucleic acid there appear 'pinpoint' or small colonies: because of their eventual disappearance after 140 generations we were not sure that they could be classified as typical mutations.

These changes have now been studied in more detail. The materials and the methods used were those described previously[1,3,4].

(1) *The nature of the changes.* It appears now that most of these changes to pinpoint colonies are typical mutations, with a varying degree of stability, that is, varying mutation-rate back to large colonies. For convenience in explaining experimental results, these mutations can be divided into three classes:

A. Typical mutations to stable 'pinpoints'. The back mutation of the latter has never been observed. The mutant 'pinpoint' strains can be maintained indefinitely by transferring from nutrient broth or nutrient agar as infrequently as every 24 hr.

B. Typical mutation to unstable 'pinpoint'. The latter back-mutate to normals which have a strong selective advantage over the 'pinpoint'; thus, any repeated transfer may eventually result in loss of the 'pinpoint' strain. It was found that the transfer of 14-hr. old (37°) pinpoint colonies containing approximately 10^6 cells still allows for the indefinite maintenance (tested for 150 generations or seven transfers) of the 'pinpoint' strain, thus proving that one deals here with a typical mutation.

We now find that 'pinpoints' of similar behaviour occur also in the spontaneously unstable Harvard strain[8] (A.T.C.C. 11887).

C. Change to 'pinpoints' so unstable that any attempt to transfer, even as early as 12 hr., results in a loss of the pinpoint 'strain'. This 'strain' may still be a typical mutant with an exceptionally high mutation-rate back to normal. If the back-'mutation'-rate approaches 1 per cell per generation, the change will usually be called a non-hereditary phenotypical change; perhaps there is no sharp dividing line between the two types of change.

The present techniques do not permit one to determine whether in each of these cases the descendants of a single cell all have only one single mutation-rate or a *whole range* of mutation-rates.

(2) *The mutation-rates.* After having established that the changes, at least in cases *A* and *B*, are hereditary, it seemed of interest to determine the rate of their occurrence (mutation-rate). The results are given in Table 1. In most cases only the lower or the higher limits of mutation-rates can be estimated from the number of colonies because of the selective advantage of the normal over the 'pinpoints'. As can be seen from Table 1, some mutation-rates for types *A* and *B* are higher than 3 per cent.

The highly unstable strain *I/Br* is of interest. Fifteen single large colonies of this strain were tested and *all* were found to produce 'pinpoints'. To ascertain that the change was indeed hereditary, three large colonies were carried through seven transfers

Table 1. Mutation-rates

Parent strain*	*I*		*I′*	*I/Br*
Unstabilizing or mutagenic agent*	5-bromouracil?	5-bromouracil deoxyriboside	5-bromouracil	none
Type of mutant*	*I/Br*	A_1–A_3 / B_1	A_4	B_2
Forward mutation-rate, per cell per generation†	4×10^{-10}	$> 10^{-3}$ / $> 5 \times 10^{-3}$	$> 3 \times 10^{-2}$	$> 3 \times 10^{-3}$
Back mutation-rate, per cell per generation†		$< 5 \times 10^{-5}$ / 1×10^{-2}‡	$< 5 \times 10^{-5}$	

* See text; the subscripts denote different mutants.

† Where the actual mutation-rate cannot be determined, the upper or the lower limits were calculated from the frequency of mutants.

‡ Determined by early restreaking.

(180 generations); after these transfers, the cells still showed unchanged unstability.

(3) *Gene unstabilization.* It appeared first of interest to study the origin of gene unstabilization in strain *I/Br*. This strain was isolated as a mutant of strain *I* grown on medium containing 5-bromouracil in the conditions of incorporation of this analogue into deoxyribonucleic acid[4]. Two other 5-bromouracil resistant mutants isolated under similar conditions proved to be as unstable as strain *I/Br*. On mutation back to 5-bromouracil sensitivity (strain *I′*), the unstability is lost. Thus, the feature of gene unstability seems to be linked with the gene for resistance to 5-bromouracil. The nature of this linkage is not clear. To date we have not succeeded in isolating a *I/Br* strain without contact with 5-bromouracil; it is therefore not known whether this gene unstabilization is a result of incorporation of 5-bromouracil into deoxyribonucleic acid (see below) or merely a selection of a spontaneous unstable mutant *I/Br*. That this latter mechanism may be operative is indicated by the fact that the mutation-rate to *I/Br* (and therefore unstability) is not increased by the presence of 5-bromouracil[4]. It is of interest that the 2-thiothymine resistant strain *I/TT*[4], which exhibits cross-resistance to 5-bromouracil, is also as unstable as *I/Br*. This strain was isolated in the presence of 2-thiothymine, which is not incorporated into deoxyribonucleic acid, but increases the content of 6-methylaminopurine in deoxyribonucleic acid (see below).

In contrast to the above, the unstabilization in strain *I* could be demonstrated only in the conditions of incorporation of 5-bromouracil into deoxyribonucleic acid (Table 1). In recent years, hypotheses have been put forward that the only action of a mutagen is gene unstabilization and that the actual mutation of such an unstabilized gene may occur automatically at some later time, without the action of a mutagen[9]. If this be applied to the case under study, one may contend that 5-bromouracil acting on strain *I* produces unstable genes at a very high rate, recognizable by the subsequent high mutation-rate (types *A* and *B*). In the mutation of type *A*, this high mutability is lost at mutation; this situation occurs often in unstable genes. On the other hand, in the mutation of type *B* (B_1), the high mutability (high back mutation-rate) persists indefinitely (tested for at least seven transfers or 150 generations). This represents, therefore, a case of induced permanent gene unstabilization.

(4) *The inducing agents.* As mentioned above, the mutations occur spontaneously in unstable strains *I/Br* and *I/TT*. It appears that the stable strains *I* and *I′* can produce frequent mutants or become unstabilized or both, under the influence of agents (Table 1) which interfere with the structure of deoxyribonucleic acid.

A. Replacement of thymine by 5-bromouracil. The 'pinpoints' (I_3 and I'_3, respectively) of type *A* and *B* can be obtained in strain *I* and *I′* (back mutant of *I/Br* to 5-bromouracil sensitivity) when grown in broth containing 100 μgm. 5-bromouracil per ml. or 125 μgm. 5-bromouracil deoxyriboside[1] per ml., and then streaked on nutrient agar. No stable 'pinpoint' colonies were observed among 5,000 colonies (strain *I′*) or 50,000 colonies (strain *I*) when 5-bromouracil or 5-bromouracil deoxyriboside was omitted.

It is of interest to note that a striking increase of mutation-rates in bacteriophage in the conditions of incorporation of 5-bromouracil into phage deoxyribonucleic acid has also been reported[10].

B. Replacement of thymine by 6-methylaminopurine[11]. This condition has been induced by growing the thymine-requiring strain *I* in basal broth containing a limited amount of thymine[1] (0·2–1 μgm. per ml.) or containing inhibiting amounts of 2-thiothymine (50 μgm. per ml.) and then streaking as usual on nutrient agar. It must be added that while 5-bromouracil produced stable (type *A*) and unstable (type *B*) 'pinpoints', the 2-thiothymine, which is not incorporated into deoxyribonucleic acid, produced only unstable 'pinpoints' (types *B* and *C*); the conditions of limited thymine produced only unstable 'pinpoints' of type *C* which cannot be as yet proved to be a typical mutation.

Two interesting cases of increase (up to 100-fold) in the spontaneous mutation-rate (to histidine independence or to streptomycin resistance) under conditions of thymine deficiency have been reported recently[12,13]; in both cases, however, the increased mutant frequencies ($2{\cdot}5 \times 10^{-5}$ or $2{\cdot}5 \times 10^{-9}$) still remained low.

The 6-methylaminopurine content has been reported to increase from 2 per cent (of thymine) up to 30 per cent, under conditions where an analogue inhibits the synthesis of deoxyribonucleic acid, but is not incorporated into it[11]. It was of interest to study whether this content increases also under conditions where the inhibitory analogue is incorporated into deoxyribonucleic acid, as in the case of 5-bromouracil. A preparation of deoxyribonucleic acid containing 42 per cent of 5-bromouracil (as referred to thymine) and a control preparation of the acid containing no 5-bromouracil were isolated and analysed as described previously[3]. The content of 6-methylaminopurine in the acid containing 5-bromouracil was not significantly higher (2·7 per cent) than that of control acid (2·0 per cent). It is concluded that when the cell can partially counteract the inhibition by building the analogue into deoxyribonucleic acid, it may do so in preference to increasing the content of 6-methylaminopurine, especially if there is no lack of thymine or thymidine (enriched nutrient broth). Thus, the genetic phenomena observed here when growing cells on 5-bromouracil seem to be due to the effect of this compound rather than to 6-methylaminopurine.

(5) *The nature of 'pinpoint' colonies.* The occasional appearance of hereditary '*G* type', 'small' or 'pin-

point' colonies in many bacterial species has been frequently reported in the past. Such colonies occurred spontaneously or as a result of treatment with agents such as barium chloride, lithium chloride, Cu++, dyes, antibiotics or just ageing. In a few recent reports where a genetic analysis was made[14,15], it was found that the 'small' mutants were present in natural populations in proportions of the order of one cell per 50,000, but were selected for by the particular agent used and therefore appeared in high proportion after treatment with this agent. However, it would be premature to say that this is true of all the agents used, that is, that indeed none of them is a mutagenic agent.

The present study was thus far made primarily with strains *I* and *I′* and their stable 'pinpoint' mutants (type *A*): the unstable pinpoints (types *B* and *C*) are difficult to study because of the high mutation-rate to 'large'.

In the case of strains *I/Br* and *I/TT* the mutation to 'pinpoint' colonies is spontaneous and therefore no selective agent is involved; on the other hand, in the case of 'pinpoint' colonies produced in strain *I′* and *I* by the action of 5-bromouracil and 5-bromouracil deoxyriboside, it was first important to ascertain that the appearance of a high proportion of 'pinpoint' colonies is not due to selection by these agents. This was indicated by the 'reconstruction experiments' in which mixtures of 'pinpoint' and normal cells (containing approximately equal proportions of each) were used as inocula. In the case of strain *I′*, the cells were allowed to multiply 10^5-fold in the presence of 100 μgm. of 5-bromouracil per ml. After 24 hr., the proportion of 'pinpoints' decreased to 3 per cent, which is of the same order of magnitude as the proportion obtained by the action of 5-bromouracil itself when no 'pinpoints' were in the inoculum. In strain *I*, the cells were grown 24 hr. in unmodified enriched medium whereupon a 4-fold volume of fresh medium containing 200 μgm. of 5-bromouracil deoxyriboside per ml. was added and the incubation continued for another 24 hr. Under these conditions, there was merely a 2-fold increase in the number of cells; this, as checked by a 'reconstruction experiment', did not change the proportion of added 'pinpoints'. Both experiments (strain *I* and *I′*) were also repeated with the 'pinpoint'-free inocula, and in this case the 'pinpoints' appeared as mentioned before.

These results indicate that in both strains the appearance of 'pinpoints' is a result of the mutagenic or unstabilizing action of 5-bromouracil or 5-bromouracil deoxyriboside and not selection.

By direct observation of dividing cells on nutrient agar and by actual cell counts in nutrient broth it was found that the lag period of stable 'pinpoint' cells (from strain *I′*) was not longer than 2 hr.; the main factor responsible for the smaller size of the colony was the 1·4 times longer division time during the logarithmic growth.

A preliminary study of the nature of the difference between the two types of cells has been made. In the case of yeast, the thorough study of Ephrussi *et al.*[16] revealed that his "petit" mutants are deficient in certain respiratory enzymes. In our case, the study of the stable 'pinpoint' mutants from strains *I* and *I′* of *E. coli* revealed that it is unlikely that the mutation is of the respiratory nature: contrary to the results of Ephrussi *et al.*, the growth of the 'pinpoint' mutants of *E. coli* is just as depressed by the lack of oxygen (Brewer's agar under anaerobic conditions) as is the growth of the normal strain. It occurred to us that the poor growth of these mutants on regular nutrient agar ('Difco') may signify that they represent various complete or partial auxotrophs for substances present in this medium in sub-optimal quantities. That various types, rather than one type, of auxotroph occur can be expected if the inducing agent (5-bromouracil) enters a variety of molecules of deoxyribonucleic acid. As mentioned above, an unstable 'Harvard' strain studied by Goldstein and Smoot[8], which is known to produce a high proportion of mixed auxotrophs, also forms a high proportion of colonies of smaller size on nutrient agar. When our 'pinpoint' mutants from strain *I′* were tested in several media, the results obtained (Table 2) indeed clearly indicated that, at least in this case, we are dealing with a nutritional mutant having perhaps multiple and/or partial growth requirements. The required additional nutrients seem to be present in yeast supplement *A* ('Difco') and are partially destroyed by boiling (½ hr.). In tryptone–peptone–yeast extract ('Difco') strain *I′* cannot be distinguished from normal.

Table 2. Study of Additional Growth Requirements of the Stable 'Pinpoint' Mutant *I′*, as compared with its Parent Strain *I′*

Strain			*I′*	*I′*,
Differential count*, per cent			75–97	0–25
Agar	Colony diam., mm., 24 hr.	Nutrient	1·5–1·6	0·3–0·6
		Nutrient + yeast suppl.†	1·5–1·6	0·8–1·2
Broth	Cell ratio‡ 24 hr.	Nutrient	0·61	
		Basal§	0·61	
		Basal + fresh yeast suppl.†	0·98	
		Basal + boiled yeast suppl.†	0·89	
		TPY‖	1·08	

* Number of colonies appearing on basal agar (ref. 17) as a percentage of those on nutrient agar ('Difco').

† Supplement *A* from yeast cells ('Difco').

‡ Ratio of cell number in a 'pinpoint' mutant culture *I′*, to that in a corresponding parent strain of the same age.

§ Basal broth (ref. 17).

‖ Tryptone, peptone and yeast extract ('Difco').

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LETTERS TO THE EDITORS

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Remanent Magnetism of the Rajmahal Traps of North-Eastern India

Following a palæomagnetic survey of the Deccan Traps of India[1–3], we have extended our investigations of Indian rock formation to other geological ages. In particular we have made rock magnetic measurements on samples of lavas from the Rajmahal Traps of eastern Bihar. These lavas, which consist mainly of basaltic and pitchstone trap flows, overlie the Upper Gondwana shales and sandstones. They are most probably Jurassic in age, and according to Hobson[4] they represent the earliest part of the volcanic activity that afterwards, during the late Cretaceous and early Eocene periods, gave rise to the Deccan Traps.

During January 1957 we collected samples of the rocks from five localities spaced about 20 miles apart. Approximately ten samples were taken from each site, usually over a distance of several hundred yards. In two of the localities the material was found in the beds of streams, while in the remaining three the blocks were obtained from freshly exposed rock in quarries. The samples were analysed in Bombay and London.

The specimens taken from the three quarries (33 blocks in all) were found to be consistently magnetized along an axis directed towards the west of north with a steep upward dip. A total of 16 blocks from the two stream beds, on the other hand, were badly weathered and showed widely scattered directions of magnetization, and the results for these particular samples have been rejected.

The magnetic direction for the samples from the quarry sites are plotted in Fig. 1 and details of the measurements are given in Table 1. The steep upward dip (−64°) is consistent with the assumption that the Indian land mass occupied a position south of the equator at the time when the rocks acquired their magnetization, and if the mean direction of magnetization is taken to correspond to that of a geomagnetic axial dipole field at the time when the rocks were formed, we must suppose that the Rajmahal area had a southern latitude of 46° at that time. The corresponding ancient position of the north geographical pole, computed on the polar wandering hypothesis, lies in the southern Carribbean at lat. 13° N., long. 70° W., as shown by point IX in Fig. 2.

It is interesting to compare this result with those published previously[1–3,6] for the lavas of the Deccan Trap series. The north pole positions obtained from measurements made on rocks from eight different sites in the Deccan formation are also shown in Fig. 2. They fall into three categories:

(1) Sites V–VIII, which belong to the Lower Traps, give pole positions farthest to the south. The rocks are all reversely magnetized (that is, in south-easterly directions) with rather steep downward dips.

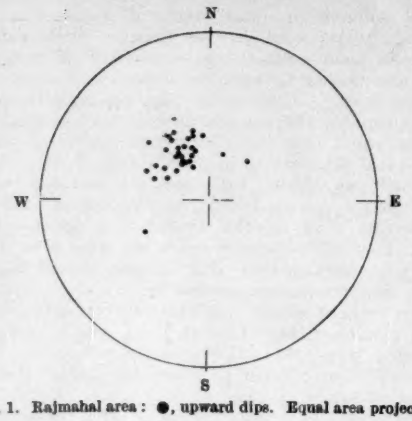

Fig. 1. Rajmahal area: ●, upward dips. Equal area projection

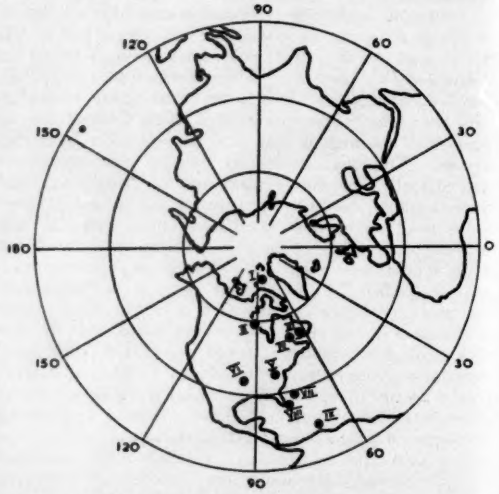

Fig. 2. Ancient pole positions for various sites: I, Pavagadh (upper); II, Pavagadh (lower); III, Amba (upper); IV, Nipani (upper); V, Linga; VI, Nipani (lower); VII, Amba (lower); VIII, Khandala; IX, Rajmahal

(2) Sites II–IV are all close to the tops of the Western Ghats, and give pole positions closer to the present geographical pole. The samples from these sites have normal magnetization (that is, towards the west of north), with considerably shallower upward dips.

(3) Site I is close to the top of Mount Pavagadh, and the rocks are acid tuffs believed to be more recent than the lavas of the main Trap sequence. Again the magnetic directions are normal, but the dips are almost zero.

It has been pointed out that this continuous decrease of dip with altitude above sea-level may be due to any one of three causes, namely:

(*a*) It may have occurred as a result of some mechanical effect associated with different depths of burial, such as that described by Graham[7].

Table 1. Rajmahal Rocks

Mean latitude of area	Mean longitude of area	No. of blocks	Mean intensity (c.g.s.u.)	Mean azimuth	Mean dip	Radius of 50 per cent circle of confidence*
25° 0′ N.	87° 51′ E.	33	$8{\cdot}85 \times 10^{-3}$	N. 328° E.	− 64° up	2°

* Measured by Fisher's method (ref. 5)

(*b*) It may be indicative of partial magnetic instability which would cause a pulling of the magnetic directions towards that of the present Earth's field. This would tend to increase the magnetic dips of the reversely magnetized rocks (1) and to decrease those of the normally polarized ones (2) and (3).

(*c*) It may represent a continuous northward movement of India relative to the equator during the time when the Deccan Traps were being erupted.

The Rajmahal lavas discussed in this communication are older than the Deccan Traps. They have steeper magnetic dips, and give a pole position (IX) farther from the present pole than any of the Deccan rocks. Considering this steeper dip in relation to postulates (*a*), (*b*) and (*c*) above, it appears that: (*a*) it is unlikely to have been caused by compressional forces associated with deep burial, since all the specimens collected came from the uppermost 250 ft. of the Rajmahal series, and it is improbable that they have ever been overlain by any substantial thickness of material[4]; (*b*) it cannot have been increased by magnetic instability in the present Earth's field, as the Rajmahal rocks are normally magnetized, and this effect would tend to reduce rather than to increase their magnetic dips.

It seems, therefore, that the combined results for the Rajmahal and Deccan Traps conform best with the postulate (*c*) and are compatible with the supposition that there was a continuous northward drift of the Indian land mass extending from the Jurassic to the Eocene.

This conclusion must be regarded as tentative pending a more detailed survey of the areas concerned. The work is continuing, and a full account will be given elsewhere.

J. A. Clegg

Dept. of Physics,
Imperial College, London.

C. Radakrishnamurty
P. W. Sahasrabudhe

Tata Institute of Fundamental
Research, Bombay.

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The Triple Point of Argon as a Thermometric Fixed Point

In connexion with experimental work on the thermodynamic properties of binary mixtures of condensed gases, it was necessary to know accurately the triple point of argon on the absolute scale. Michels, Wassenaar, Sluyters and De Graaff[1] have recently made a careful determination of this temperature using a platinum resistance thermometer. They suggested that it should be used as a fixed point for thermometer calibrations, and since this proposal has much to recommend it, it is desirable that there should be an independent estimate of the triple point.

Using cylinder gases of the highest purity obtainable, further purified by fractional distillation, we have measured the vapour pressure of oxygen at the triple point of argon, the liquid oxygen being contained in a small bulb surrounded by argon which had just started to melt. The manometer used was that described by Lambert and Phillips[2], and individual measurements of the oxygen vapour pressure at the argon triple point were constant to $\pm$ 0·01 mm. The average value was 368·69 int. mm. of mercury with an estimated absolute uncertainty of $\pm$ 0·05 mm. Hoge[3] has given values of the vapour pressure of oxygen on a temperature scale based on a helium gas thermometer on which the oxygen boiling point is taken as 90·190° K. (with 0° C. $\equiv$ 273·160° K.). In the range involved, Hoge gives oxygen vapour pressures accurate to 0·1–0·2 mm., corresponding to temperatures accurate to 0·003–0·006°. From his data and the above figure for the oxygen vapour pressure, the triple point of argon is 83·819 $\pm$ 0·006° K. This is in good agreement with the value of Michels *et al.* of 83·819 $\pm$ 0·001° K.

Aston and Moessen[4] have recently carefully determined the normal oxygen boiling point and obtained a value of 90·154° K., which is 0·036° lower than the figure of the 1948 International Temperature Scale. With their value of the oxygen boiling point, our value for the argon triple point is 83·783 $\pm$ 0·006° K.

We have also made repeated measurements of the triple point pressure of argon, obtaining an average value of 516·82 $\pm$ 0·04 mm. The figure of 515·7 mm. given by Clark, Din and Robb[5] and used by them to obtain the triple point is undoubtedly too low.

R. A. H. Pool
B. D. C. Shields
L. A. K. Staveley

Inorganic Chemistry Laboratory,
Oxford. Dec. 20.

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Raman Spectrum of Tetrakistrifluorophosphine-nickel

We have observed the Raman spectrum of liquid tetrakistrifluorophosphine-nickel, $Ni(PF_3)_4$, prepared as described by Wilkinson[1]. As required for the point group T_d, all the lines of the spectrum are found to be depolarized except three, which are highly polarized ($\rho \sim 0$). This observation provides strong evidence that the arrangement of the four attached phosphorus trifluoride molecules around the nickel atom is regular tetrahedral (cf. nickel tetracarbonyl).

The Raman frequencies of the three polarized lines are 954, 534 and 195 cm.$^{-1}$. They are clearly to be assigned respectively to ν_1 (mainly P–F stretching), ν_2 (mainly symmetrical 'umbrella' PF_3 deformation) and ν_3 (mainly Ni–P stretching). For comparison, the values of ν_1 and ν_2 for the phosphorus trifluoride molecule[2] are respectively 890 and 531 cm.$^{-1}$. Assuming a simple valency force field, the three polarized frequencies of $Ni(PF_3)_4$ may be used for deriving the two bond-stretching force constants k_{P-F} and k_{Ni-P} and the deformation force constant k_δ/l^2. The values thus obtained are $k_{P-F} = 7{\cdot}73$, $k_\delta/l^2 = 0{\cdot}53$ and $k_{Ni-P} = 2{\cdot}71$ (all in units of 10^5 dynes per cm.). The first two may be compared with the corresponding values for the phosphorus trifluoride molecule, $k_{P-F} = 5{\cdot}38$ and $k_\delta/l^2 = 1{\cdot}15$, as calculated from ν_1 and ν_2 assuming simple valency forces. The force constant of stretch-

ing of the bond between the central nickel atom and each of the attached phosphorus atoms is of particular interest. The corresponding value of k_{Ni-C} for nickel tetracarbonyl, likewise calculated[3] on the basis of a simple valency force field, is 2·52. Both k_{Ni-P} and k_{Ni-C} are in the range normally expected for single bonds, and are thus remarkably low in view of the other evidence which has led to the conclusion[1,4] that nickel–ligand bonds of this kind have appreciable double-bond character.

L. A. Woodward
J. R. Hall

Inorganic Chemistry Laboratory,
University of Oxford.
Feb. 5.

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Retinal Type of Photovoltaic Cell

It is known that the retina of the animal is a photovoltaic mosaic composed of countless minute individual light receptors. The gradual development of multiple electrode photovoltaic cells may perhaps lead to an actual working model of the retina.

Previously constructed multiple-electrode photovoltaic cells have the seat of their photopotential within each single electrode unit. A typical example of a wet multiple photovoltaic cell is the telephotographic system of Ruben[1], which utilizes the copper/cuprous oxide barrier-layer in each individual receptor unit. Photoemissive mosaic cells such as the television iconoscope, etc., obviously do not approach the retina in principle.

The cell described here is unlike previous multiple-electrode wet photovoltaic cells in that the individual electrodes are completely inert; the seat of the photopotential is apparently in the space charge formed in the volume around the point electrode during irradiation[2]. Certain organic structures when irradiated in solution produce photopotentials with respect to the unirradiated solution, a development from the Becquerel effect. The point electrode here is a potential probe.

The construction of the multiple cell, together with the irradiation box, is shown in Fig. 1. The cell can have as many points as desired on any shape of surface, and only one common dark electrode is required. In Fig. 1, nine point electrodes are shown mounted in a piece of cylindrical 'Bakelite' coil form which is cemented to a glass slide. The bottom electrode is made the common dark electrode. Platinum wire 0·02 in. in diameter is used for all the electrodes. The exposed platinum wire ends inside

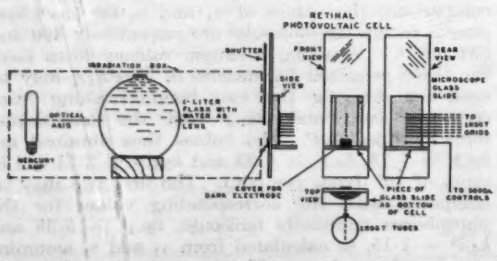

Fig. 1. Retinal photovoltaic cell and irradiation box

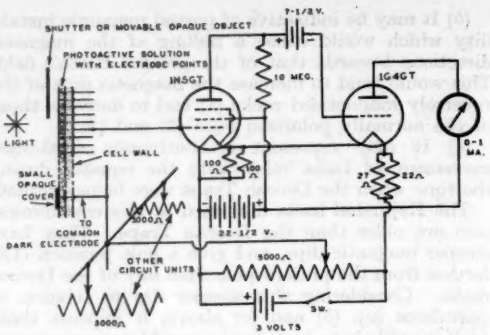

Fig. 2. Amplifier 'brain' unit attached to one electrode point

the compartment are about 0·05 in. long and can be assumed to be points. The cell is cemented together with an epoxy adhesive. A small strip of black electrical or masking tape hides the bottom point from light. The cell is filled with about 0·25 c.c. of a 1 per cent solution of benzoin in 95 per cent ethyl alcohol as one of a series of photoactive solutions[3]. A photopotential as high as 0·15 V. may be generated by each irradiated point with this solution; but in general, sensitivity of the point electrode to the space charge surrounding it during irradiation is more important than the magnitude of photopotential if an analogy to the retina is to be drawn. The wave-length response of the cell is dependent on the photo-active solute used.

The cell obviously requires an auxiliary apparatus to indicate that the cell is actually seeing light and a moving object. Fig. 2 shows a simple battery-operated electronic amplifier actually used for each irradiated electrode point. Each milliammeter indicates what its respective point is seeing—light or darkness.

The analogy between the rods and cones of the retina and the point electrodes of this photovoltaic cell, the roles of visual purple and the photo-active solution, and the requirement of a common dark electrode or 'ground' are accented with discretion.

Irvin Levin

Instrumentation Division,
Walter Reed Army Institute of Research,
Washington, D.C.
Dec. 26.

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Contrast in the Electron Microscope Image

The detail seen in a highly magnified electron micrograph is to-day limited more by a lack of contrast in the image than by any lack of resolution in the microscope, which is now usually capable of resolving objects at least as small as 15 A., that is, only a few atoms in diameter. Contrast may be increased either by metal-shadowing the surface of the specimen or by treatment with various reagents. By analogy with the use of stains in light microscopy this latter technique has come to be known as 'electron staining' (perhaps this is an unfortunate, if convenient, term since the analogy is not very

close). However, it is still not quite clear what physical property makes a substance likely to be useful for increasing contrast. Published statements say variously that high density, high atomic number, high atomic weight or the presence of atoms of a metal are required. Even the extreme claim that all substances are almost equally efficient in increasing electron density has been argued.

It is unfortunate that exact measurements of contrast in the electron image are not easy to make, and subjective judgments are unreliable except in the rare cases of obviously intense staining. A simple theoretical approach to the problem of contrast has recently been made by Zeitler and Bahr[1], and since this is in reasonable agreement with the few satisfactory measurements that have so far been published[2,3] it seems worth deducing from their formulæ the conditions for a just appreciable increase in contrast in the image of any part of a specimen. If the part considered covers an area S cm.^{2} at right angles to the beam, then we may deduce that a distinct increase in contrast requires the addition of N atoms of atomic number Z uniformly over or through the object where $NZ^{4/3}/S = 5{\cdot}5 \times 10^{17}$ when the microscope works with a high-tension voltage of 60 kV. ($4{\cdot}7 \times 10^{17}$ for 50 kV.; $8{\cdot}25 \times 10^{17}$ for 100 kV.). A reasonably contrasting photographic recording of the image and a small objective aperture have been assumed. This simple formula allows one to predict the effect of adding various amounts of different substances to the specimen and also, in a measure, to reconcile some of the conflicting views on the requirements for contrast.

There are three useful bases on which to compare contrast, and it is important to distinguish which is relevant in any particular case.

(1) Contrast compared on the basis of equal thickness. This is particularly relevant when selecting a material for shadowing the specimen where an adequate increase in contrast with the minimum thickness of deposit is required. If d cm. is the thickness of the thinnest film of material that gives increased contrast, ρ gm.cm.$^{-3}$ the density of the film, A its atomic weight and L Avogadro's number ($6{\cdot}02 \times 10^{23}$), then $NZ^{4/3}/S = \rho dLZ^{4/3}/A = 5{\cdot}5 \times 10^{17}$, that is, $d = 0{\cdot}915 \times 10^{-6}\,(A/Z^{4/3})\,(1/\rho)$. Now $A/Z^{4/3}$ is nearly unity for all elements (it varies from 0·57 for uranium to 1·6 for lithium), so that, judged on the basis of equal thickness, the maximum contrast is given by the material with the greatest density. In the particular case of platinum ($A/Z^{4/3} = 0{\cdot}59$, $\rho = 21{\cdot}5$), $d = 2{\cdot}5$ A., which agrees well with practice, where an estimated 5 A. is found to be a suitable thickness for the film of metal-shadowing. This calculation also shows that a useful histochemical technique for the electron microscope might be that of forming small deposits of some dense substance specifically in the tissue; indeed, the few successful techniques so far have been mostly of this type[4,5].

(2) Contrast compared on the basis of equal weight. If an object d cm. thick of density ρ gm.cm.$^{-3}$ can bind p per cent by weight of an added substance, then a detectable increase of contrast requires p to have a value of not less than that given by $(p/100)\,\rho dLZ^{4/3}/A = 5{\cdot}5 \times 10^{17}$, that is (taking $Z^{4/3}/A$ and the density of the specimen as unity), $p = 9 \times 10^{-5}/d$.

We thus obtain the unexpected result that for a specimen of given thickness the percentage uptake of stain required for contrast is almost independent of the nature of the stain used. For an object about 0·5μ thick (say, a bacterium) a 2 per cent uptake of any stain should increase contrast, and it might be worth trying the usual light microscope stains on such objects viewed with the electron microscope. On the other hand, for a thin section (200 A.) 45 per cent of stain is required. A similar calculation has led Ornstein[6] to claim that, since osmium is only taken up to the extent of about 10 per cent, osmium staining of thin sections must be a fiction. As shown later this conclusion may not be altogether justified, and it certainly conflicts with a deep-set belief of many electron microscopists.

(3) Contrast compared on the basis of equal numbers of molecules. If the object to be stained is d cm. thick and can bind n molecules of stain per cm.^{3}, then for increased contrast $nd(\Sigma Z^{4/3}) = 5{\cdot}5 \times 10^{17}$ where $(\Sigma Z^{4/3})$ is obtained by adding up the values of $Z^{4/3}$ for each atom in the molecule of stain. It is on this basis that different stains would normally be compared. If, as may often be the case, the molecules of stain are bound to the surface of membranes and not uniformly distributed throughout the tissue as has been assumed above, then if n molecules are bound per cm.^{2} of membrane surface, the normal to which makes an angle θ with the beam at any point, contrast results when $n(\Sigma Z^{4/3})\sec\theta = 5{\cdot}5 \times 10^{17}$. If the tissue is fixed with osmium tetroxide and this is bound on the membranes as the lower oxide OsO_2($\Sigma Z^{4/3} = 353$), then when $\theta = 89°$ (that is, in a section where part of a membrane runs almost at right angles to the surface) $n = 2{\cdot}5 \times 10^{13}$ cm.$^{-2}$, or one atom of osmium bound per 400 A.^{2} of the membrane surface will increase its contrast. This is a very plausible figure, so that both in theory and in practice osmium staining of the surface of membranes may well be a fact.

Robin C. Valentine
National Institute for Medical Research,
Mill Hill,
London, N.W.7.
Feb. 5.

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Sintering of Tin Powder

Most powdered metals will sinter together appreciably when heated to a temperature which, in the absence of extraneous factors, is about three-quarters of their absolute melting point; metals of low melting point tend to be exceptions, however, to this generalization in that they can be heated nearly to their melting point before sintering takes place. There is no evidence, in fact, that tin has ever been observed to sinter[1]. Tin powder is usually made by the atomization of the molten metal and particles of the powder become coated with a film of stannic oxide; this oxide cannot be reduced below the melting point of tin by any atmosphere normally used in sintering. It has already been found[2] that sintering previously pressed powder leads to no further densification of the compact and confers no beneficial properties on it. The inability of tin to undergo sintering has been generally assumed, but never shown, to be due to the presence of unreduced oxide films.

It was decided to test this assumption by attempting to sinter loose oxide-free powder. To obtain

such powder, massive white tin was transformed to the grey allotrope at −40° C. in an evacuated and 'gettered' glass tube. Considerable volume changes occur during transformation, and the grey tin formed as a more or less granular powder. This loose powder was re-transformed to white tin at room temperature and shaken to one end of the tube. It was then heated at 220° C., for 48 hr.

The powder had formed a coherent, but friable, mass, which was removed from the tube in one piece. Specimens from this were prepared for micro-examination, which showed much evidence of growth across particle boundaries, indicating that sintering had taken place. It is, of course, to be expected that sintering would take place much more rapidly and completely if the powder were first compacted, but it may be concluded that, in the absence of oxide films, tin powder will sinter when heated.

R. F. Smart
E. C. Ellwood

Tin Research Institute,
Fraser Road,
Perivale,
Greenford, Middlesex.
Feb. 4.

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Thermoelectric Properties of Bismuth Telluride and its Alloys

Previous publications from this Laboratory have described the progress made with bismuth telluride in connexion with thermoelectric refrigeration or generation. With a thermo-junction between p-type bismuth telluride and bismuth the maximum temperature difference obtained[1] was 26° C., and between p- and n-type bismuth telluride[2] 40° C. This corresponded with a figure of merit $\eta\sqrt{\sigma/K}$ of 0·0335 degree$^{-1/2}$. Here η is the thermoelectric power, σ the electrical conductivity, and K the thermal conductivity.

In our work on this compound in 1956 we were able to make p- and n-type bismuth telluride with greater uniformity and to realize more exactly the optimum thermoelectric power, which is near 200 μV./deg. C. The outcome of this was a maximum temperature difference of 65 deg. C. at a mean temperature of 17° C. The physical properties of the material are consistent with those reported by me at the Garmisch conference in September 1956[3], except that Hall electron mobilities as high as 300 cm.^{2} volt^{-1} sec.$^{-1}$ have been obtained in more recent material.

A temperature difference of 65 deg. C. has been quoted recently by Shilliday[4], of the Battelle Laboratories. In Russian work, Sinani and Gordyakova[5] have quoted a figure of merit of 0·049 for one component of a junction. This was n-type, and consisted of an alloy of 80 per cent bismuth telluride and 20 per cent bismuth selenide. The highest temperature difference quoted in Russian work[6] appears to be 70 deg. C., indicating that their p-type material is inferior to the n-type quoted, since if both components of a junction have a figure of merit of 0·049, the maximum temperature difference is larger than 70 deg. C., as indicated later.

It is to be expected, as discussed by Joffe[7], that in alloys the thermal conductivity will be less than that of either single component, because the substituted atom causes phonon scattering. In favourable circumstances this can be achieved without appreciably increasing the electron scattering. In accordance with this, we have recently studied alloys of bismuth telluride – bismuth selenide, bismuth telluride – antimony telluride, bismuth telluride – bismuth sulphide and also alloys of bismuth telluride – antimony telluride containing either selenium or sulphur. With some compositions both p- and n-type material with the optimum thermoelectric power has not yet been obtained, and in fact this is the case with alloys giving the lowest thermal conductivity. Taking the most favourable n- and p-type alloys so far prepared, we have been able to obtain a mean figure of merit of 0·049 for the junction, corresponding with a maximum temperature difference of 80 deg. C. for a mean temperature of 17° C. This results primarily from the fact that substitution of antimony for bismuth lowers the lattice component of the thermal conductivity, K_0, from 0·0157 for bismuth telluride to a minimum of 0·010 ohm. cm.$^{-1}$ deg. C.$^{-1}$ at about 50 atomic per cent of antimony, while the electron mobility is not reduced. Substitution of sulphur or selenium for tellurium in any $(Sb–Bi)_2–Te_3$ alloy causes a further small reduction in K_0, but the amount which can usefully be substituted is restricted because of electron scattering.

Our work on the optical properties[8] indicates that on substituting selenium for tellurium in bismuth telluride, there is a change in band structure near the composition Bi_2Te_2Se, which may be associated with the decrease in mobility on adding selenium because of an increase in inter-valley scattering. We have not so far been able to obtain infra-red transmission through $(Bi–Sb)_2–Te_3$.

I would like to acknowledge the collaboration of several scientists working as a team on this subject. A. R. Sheard is responsible for preparing the material, H. J. Goldsmid for studying thermal conductivity and thermoelectric power, J. R. Drabble for investigating the galvanomagnetic effects, and I. G. Austin for examining the optical properties. P. A. Walker and B. Yates are extending the measurements down to liquid-helium temperatures. In addition to the papers referred to above, some of the more recent work has been published[9], and other papers are in preparation[10].

D. A. Wright

Research Laboratories,
The General Electric Co., Ltd.,
Wembley, Middlesex.

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Mechano-Chemical Anodic Dissolution

Hoar and Hines[1–5] have studied the transgranular stress-corrosion cracking of 18–8 type stainless steels in aerated aqueous magnesium chloride solution (42 wt.-per cent of magnesium chloride) at temperatures of 120–154° C., using thin wires directly stressed in tension. They found rates of crack propagation in the range 0·5–2 mm./hr., similar to those found in other work using much thicker material, and several orders of magnitude smaller than the rate of cracking in mechanical brittle fracture. They suggested that a crack is propagated by the selective anodic dissolution of the highly stressed metal forming its advancing edge, the unstressed sides of the crack remaining relatively unattacked.

By measurements of the electrode potential of cracking wires, and of potential and current density on unstressed wires polarized cathodically with impressed e.m.f., they estimated the current flowing from the surface oxide-film cathode into each crack as of the order of 0·5 μamp.; this, together with the anodic current-density at the advancing edge necessary to give the observed rate of crack propagation by metal dissolution to Fe^{II}, Cr^{III} and Ni^{II}, say 0·4–1·6 A./cm.², leads to reasonable values of the width of the advancing edge, some tens or hundreds of angstroms. The potential measurements also showed that the postulated anodic dissolution of stressed metal at high current density must take place, if it in fact occurs, with only a few millivolts polarization of the anode above its zero-current potential of about −0·15 V. (*N*-hydrogen scale). While there are no grounds for supposing that stressed or even strained metal is, *per se*, much more easily dissolved anodically than unstressed metal, it seemed possible that metal in the act of yielding under stress might be so. Measurements of the overall extension of wires during cracking suggested that the actual strain-rate at the advancing edge of each crack may well be of the order of 10–100 per cent/sec., and it was thus of interest to see whether a macroscopic area of 18–8 type stainless steel yielding at a comparable rate could sustain a high anodic current density of the order of 1 amp./cm.², leading to equivalent metal dissolution, at very small polarization.

We have modified the apparatus of Hoar and Hines[1–5] so that in it a wire may be extended very rapidly while it undergoes anodic polarization in a hot aqueous solution. Arrangements for the simultaneous measurement of extension-rate, electrode potential and current are included, and for the rapid flow of the solution past the wire if required.

When an unstressed wire of 18–8 steel (carbon, 0·09; silicon, 1·00; manganese, 0·78: chromium, 17·1; nickel, 9·2; molybdenum, 0·18; copper, 0·2; nitrogen, 0·06; niobium + tantalum, titanium, <0·05 wt.-per cent) was polarized anodically in stagnant 42 wt.-per cent aqueous magnesium chloride solution at 154° C. so that the current density was 0·5 amp./cm.², the potential was +0·52 V. and the wire dissolved uniformly over its whole surface to Fe^{II}, Cr^{III} and Ni^{II} with about 100 per cent current efficiency. When a similar wire was treated similarly during its extension at 5 per cent/sec., the potential was + 0·35 V. and similar uniform dissolution occurred. The rapid macroscopic dissolution produced, however, considerable concentration polarization caused by the accumulation of dissolved metal ions in the liquid adjacent to the wire. To reduce this, further experiments, in which similar wires were extended at 5 per cent/sec. while dissolving at 0·5 amp./cm.², were made with the solution forced at 50 cm./sec. past the wire (which was held in the axis of a 1-cm.-diam. glass tube): the potential was now − 0·15 V., very close to the zero-current potential of unstressed 18–8 steel anodes and slightly more negative than the potentials observed during stress-corrosion cracking[1–5], the wire again dissolving uniformly.

These experiments demonstrate that 18–8 steels can dissolve anodically at high current density with very small polarization provided that the surface is yielding rapidly in a direction normal to the direction of dissolution. They provide strong confirmation for the hypothesis of Hoar and Hines for the mechanism of crack propagation; as we shall show in a full report of this work elsewhere, concentration polarization at a very small anode at the advancing edge of a yawning crack is likely to be very small. The experiments also provide an example, on the macroscopic scale, of a mechano-chemical effect: an anodic dissolution that is highly stimulated by the mechanically induced movement of the surface metal atoms.

Similar experiments on pure nickel gave no indication of mechanical stimulation of its anodic dissolution in hot aqueous magnesium chloride solution; significantly, nickel does not suffer stress-corrosion cracking under these conditions. Although 18–8 steel and nickel both have face-centred cubic lattices, the number, distribution and mobility of dislocations and other zones of disarray are clearly different in the alloy and in the pure metal, which may well account for the difference in their behaviour when they undergo simultaneous yielding and dissolution.

T. P. Hoar
J. M. West

Department of Metallurgy,
University of Cambridge.
Jan. 29.

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Electronic Configuration of Cations in some Distorted Spinels

Two apparently different approaches have been used for explaining the distortion to tetragonal symmetry of some spinels. Both, however, rely on the behaviour of cations having 3*d* electrons. Thus in one the origin of distortion is attributed to the ability of these cations (for example, Mn^{3+}) to form the appropriate covalent bonds with the overlapping oxygen orbitals[1–3]. On the other hand, the second approach relates it to the crystal field effects and Jahn–Teller distortions[4] of ligands around such cations[5].

In what follows the observed electronic configuration of cations in a few cases is examined in the light of these schemes. The analysis of the available magnetic data for $Mn^{2+}[Mn_2^{3+}]O_4^{2-}$ and for the γ-oxide $(Mn_{\frac{8}{3}}^{3+}O_3^{2-})$ showed that Mn^{3+} (d^4) ions at octahedral sites in both the substances have four unpaired *d*-electrons. However, the tetrahedrally situated cations, that is Mn^{2+} (d^5) in the former and

Mn^{3+} (d^4) in the latter had only one and two unpaired electrons respectively[3]. This distribution satisfies the requirement of Mn^{3+} ions at octahedral sites which in virtue of one empty d-orbital would take part in dsp^2 square bonds to produce the appropriate distortion of each elementary octahedron ($c/a>1$). Furthermore, the cations at tetrahedral sites which have two empty d-orbitals would seem to form d^2sp hybrid (irregular tetrahedral) bonds[3]. This is also in accord with the geometrical requirements where in both the substances the MeO_4 tetrahedra are also distorted and can be described as tetragonal sphenoids[4] with $\alpha_1 = \alpha_2 = 102° 30'$ and $\beta = 113° 34'$, the central cation being equidistant from oxygen ions.

The above electronic distribution of $3d$ electrons in the tetrahedrally situated cations, which seems to be in violation of Hund's rule, can also be justified from the point of view of crystal field theory.

According to this approach[5], the $(t_{2g})^3$ (e_g) configuration of octahedrally situated Mn^{3+} (d^4) ions, having degeneracy with respect to e_g orbitals, gives rise to a Jahn–Teller distortion whereby $c/a>1$. This is in agreement with observation. However, configurations with highest permissible multiplicity for tetrahedral cations are $(e)^2$ $(t_2)^3$ for Mn^{2+} in $Mn^{2+}[Mn_2^{3+}]O_4$ and $(e)^3$ $(t_2)^3$ for Mn^{3+} in γ-$Mn_2^{3+}O_3$. For the former the Jahn–Teller distortion is zero and for the latter $c/a<1$. In fact, this distortion of tetrahedral groups will oppose that of octahedral groups. This would conflict with the observed situation where in both the substances $c/a = 1{\cdot}16$. A distortion of the tetrahedral group consistent with $c/a>1$ is expected from a configuration where there is, in these ions, a single electron in the t_2 orbital (in effect T_2 state). Thus the modified configuration for such ions would be $(e)^4$ (t_2) for d^5 and $(e)^3$ (t_2) for d^4. This distribution is in agreement with the total spin values derived from the magnetic data.

Actually, the distortion of MeO_6 octahedra to give $c/a>1$ and the corresponding distortion of MeO_4 tetrahedra amounts to the production of a strong crystal field which would favour the low spin-states.

It is expected that a molecular orbital approach which takes into account the essential features of the Jahn–Teller effects would lead to a more consistent picture.

I am grateful to Prof. M. H. L. Pryce for discussions.

K. P. Sinha*

H. H. Wills Physics Laboratory,
University of Bristol.
Jan. 20.

* Permanent address: National Chemical Laboratory, Poona 8, India.

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Murexide Test in Paper Chromatography

Michel[1] found that the purines on a moist filter paper, when treated with chlorine gas for 3 min., dried at 80–90° C. and left in an ammonia atmosphere for a few minutes form compounds which are detectable by their fluorescence in ultra-violet light. A true murexide test, with development of a pink coloration, can be carried out without using chlorine directly, by lightly spraying the paper first with a 10 per cent solution of chloramine *T* (B.P.) and then with *N* hydrochloric acid, followed by heating it in a steam-oven (96–98° C.) until there is no smell of chlorine. The dried paper is then passed slowly over the open mouth of a bottle of concentrated ammonia and heated in the steam-oven for 5–10 min., when the purines will show up as pink spots. It is sometimes necessary to repeat the ammonia treatment and subsequent heating to make the spot visible. Morgan[2] used chloramine *T* as an oxidizing agent for detection of caffeine in the conventional way by the murexide test.

This method will detect uric acid, caffeine, theophylline, theobromine and xanthine in approximately microgram quantities of 1·5, 18, 38, 1 and 7 respectively. When viewed in ultra-violet light the spots show violet fluorescence, and the minimum detectable quantities are reduced to approximately one-third of those quoted above.

We wish to thank the Indian Tea Association for permission to publish this communication.

N. B. Chanda
S. Chakraborty

Tocklai Experimental Station,
Indian Tea Association,
Cinnamara, Assam.

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Acidity of Sprays used in detecting Ketohexoses on Paper Chromatograms

Several spray reagents are available for the specific detection of ketoses and ketose-containing sugars on paper chromatograms. Probably the most widely used are resorcinol and naphthoresorcinol, originally prepared as sprays in 2 *N* hydrochloric acid by Forsyth[1]. Afterwards, Partridge[2] replaced the hydrochloric acid with trichloracetic acid in order to minimize attack on the filter paper. Heyrovsky[3] has recently reported that β-indolyl acetic acid in trichloracetic acid is also a very sensitive ketose-specific reagent. In work in this laboratory on two fructose-containing disaccharides it has been found that all three of the above reagents, when prepared in trichloracetic acid, failed completely to detect these sugars on paper chromatograms. When the trichloracetic acid was replaced with 2 *N* hydrochloric acid strong positive tests were obtained. The results are listed in Table 1.

Disaccharide *A* was obtained from red clover (*Trifolium pratense*) and yielded fructose only on hydrolysis. Disaccharide *B* was isolated from cultures of *Streptococcus bovis*[4] and yielded both glucose and fructose on hydrolysis. Both sugars were somewhat resistant to acid hydrolysis and required 4 hr. at 100° in 1·5 *N* sulphuric acid for complete hydrolysis. The two disaccharides were applied to

Table 1

Reagent	Acid	Disaccharide *A*	Disaccharide *B*
Resorcinol	Trichloracetic	nil	nil
	2 *N* hydrochloric	bright red	bright red
Naphtho-resorcinol	Trichloracetic	nil	nil
	2 *N* hydrochloric	deep red	deep red
β-Indolyl-acetic acid	Trichloracetic	nil	nil
	2 *N* hydrochloric	strong violet	strong violet

papers as spots containing 20–25 μgm. of sugar, and the sprayed papers heated according to the methods employed by Forsyth[1], Partridge[2] and Heyrovsky[3]. No reaction was obtained with the trichloracetic acid reagents even with prolonged heating times. Fructose and sucrose, under these conditions, reacted readily with all three reagents in either acid, while glucose gave no reaction.

All three reagents appear to have been developed with fructose and oligosaccharides containing the easily hydrolysed sucrose linkage (sucrose and raffinose) as reference sugars. From the results just reported it appears that either free ketose only can react with the reagents or else a specific part of the ketose molecule may be involved in the reaction. In this latter case a stable glycosidic linkage involving this specific portion of the ketose molecule would prevent reaction with the reagents. In either case preliminary hydrolysis would be necessary. The results described suggest that trichloracetic acid, under the recommended conditions, is unable to hydrolyse the more stable glycosidic linkages and thus the stronger hydrochloric acid is necessary.

It is evident that in testing an unknown oligosaccharide for ketoses on a paper chromatogram the stronger acid sprays are necessary. A positive reaction with only the stronger acid reagent may be a useful indication of the strength and possible nature of the glycosidic linkage involved.

R. W. Bailey

Plant Chemistry Division,
Dept. of Scientific and Industrial Research,
Palmerston North,
New Zealand.
Jan. 10.

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Quantitative Determination of Hydroxyproline by Paper Chromatography

Recently, we showed that the quantitative analysis of amino-acids using the maximum colour density method could be accomplished with low errors and good reproducibility[1]. As regards the analysis of hydroxyproline, the accuracy can be considerably improved if the colour development is effected using a ninhydrin–isatin–triethylamine reagent[2] rather than isatin[1].

A solution containing 46·8 mgm. of hydroxyproline in 100 ml. of 10 per cent *iso*propanol was assayed by the paper chromatographic procedure[1]. This procedure employs strips of Whatman No. 1 filter paper, 23 cm. × 57 cm., buffered at *p*H 8·4 and a 40-hr. descending development in the solvent system *m*-cresol buffered at *p*H 8·4.

Eighteen quantitative chromatograms were prepared. Following solvent development and drying at 60° C. for 1 hr., nine of the chromatograms were dipped in 0·4 per cent isatin in water-saturated *n*-butanol containing 4 per cent acetic acid (reagent *A*). The blue colour of the hydroxyproline spots was developed by heating the dipped chromatograms for 10 min. at 100° C. and then storing at room temperature for 24 hr. The maximum densities of the spots were read using a 'Densichron' transmission densitometer (Model 3835*B*, W. M. Welch Scientific Co., Chicago), the red built-in filter, and a 3-mm. aperture.

Table 1. Accuracy and Precision of Paper Chromatographic Determination of Hydroxyproline

Chromatogram	Calculated concentration (mgm./100 ml.)	
	Reagent *A*	Reagent *B*
1	44·9	45·9
2	44·0	47·7
3	47·7	44·9
4	44·9	44·9
5	44·0	44·9
6	49·6	48·7
7	43·1	45·9
8	45·9	46·8
9	43·1	47·7
Average	45·2	46·4
Standard deviation*	2·18	1·41
Deviation from theory†	3·4%	0·9%

* Standard deviation = $\sqrt{\frac{N\Sigma X^2 - (\Sigma X)^2}{N(N-1)}}$

† Theoretical value = 46·8 mgm./100 ml.

The remaining nine chromatograms were dipped in a reagent composed of 270 mgm. of ninhydrin, 130 mgm. of isatin and 2 ml. of triethylamine made up to 100 ml. with water-saturated *n*-butanol (reagent *B*)[2]. After heating for 15 min. at 60° C., the maximum densities of the red-purple hydroxyproline spots were read using the 'Densichron' transmission densitometer and no filter.

The results of this comparative quantitative study are given in Table 1. In addition to a marked improvement in the accuracy and precision of the analysis, the use of reagent *B* (the new ninhydrin–isatin–triethylamine reagent) offers a number of other distinct advantages: (1) It is specific for hydroxyproline[2]. (2) It is more sensitive than isatin. The concentration of the standard and unknown solutions spotted on the filter paper are one-half the concentration required when using isatin[1]. (3) The maximum densities of the hydroxyproline spots are greater and can be read immediately after colour development, thus eliminating the 24-hr. colour development period necessary when using isatin.

Henry R. Roberts
Michael G. Kolor

Research Laboratories Division,
National Dairy Products Corporation,
Oakdale, Long Island,
New York.
Dec. 13.

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Iron Content of Hæmoglobin in Iron Deficiency

In iron-deficiency anæmia insufficient iron is available for hæmoglobin synthesis to proceed at a normal rate. Presumably the primary difficulty is in the synthesis of the prosthetic groups of hæmoglobin, hæm. Since the usual methods of estimating hæmoglobin (spectrophotometry, iron analysis, oxygen capacity) depend upon the hæm group, it appeared possible that the decrease in 'hæmoglobin' observed in this deficiency state might be one of hæm rather than hæmoglobin. Thus, unless globin synthesis was restrained by the absence of hæm, globin molecules with less than the normal four hæm groups might be produced. The studies reported here were undertaken to determine whether appreciable quantities of such a hypothetical incomplete pigment were present.

Table 1

Sample	No. of aliquots	Iron content as average per cent of dry weight ± standard error of the mean
Mother liquor from first crystallization of hæmoglobin from normal rats	2	0·312
Mother liquor from first crystallization of hæmoglobin from iron-deficient rats	2	0·298
Purified (twice crystallized) hæmoglobin from normal rats	5	0·3488 ± 0·0027
Purified (twice crystallized) hæmoglobin from iron-deficient rats	5	0·3364 ± 0·0053
Purified (once crystallized) hæmoglobin from normal human	4	0·3260 ± 0·0017
Purified (once crystallized) hæmoglobin from iron-deficient human	4	0·3173 ± 0·0020
Purified (twice crystallized) hæmoglobin	4	0·3328 ± 0·0028
Purified (twice crystallized) hæmoglobin from iron-deficient human	5	0·3496 ± 0·0028

Rat hæmoglobin was isolated from the heparinized blood of animals rendered iron-deficient by feeding them a mild diet supplemented with an iron-free salt and vitamin mixture. Control animals were fed an identical diet to which ferrous sulphate had been added. The hæmoglobin was crystallized by shaking an alkalinized hæmolysate with toluene, centrifuging, filtering and adjusting the *p*H to 6·8. The crystals were redissolved with the aid of dilute ammonia, and recrystallization was achieved by dialysis against large volumes of iron-free water. Prior to drying for analysis, the crystals were dissolved in iron-free water with the aid of dilute ammonia and the resultant hæmoglobin solution was filtered.

Human hæmoglobin was isolated from the blood of a normal young man and from the blood of a woman with long-standing uncomplicated iron-deficiency anæmia. Crystalline human hæmoglobin was prepared according to Drabkin's method[1]. It was washed briefly in 2·8 *M* phosphate buffer at *p*H 6·8 and was then redissolved in iron-free distilled water, filtered, and dialysed against large volumes of iron-free water. An aliquot of the hæmoglobin was recrystallized by further dialysis against 2·8 *M* phosphate buffer, *p*H 6·8, and was washed overnight in the buffer. The crystals were redissolved in 2·0 *M* phosphate buffer *p*H 6·8, filtered, and dialysed against several large volumes of iron-free water.

2–5 aliquots of each hæmoglobin solution were dried to a constant weight at 110° C. and were then wet-ashed with concentrated sulphuric acid and hydrogen peroxide. In the case of human hæmoglobin solutions, residual potassium phosphate represented less than 1 per cent of the dry weight as estimated by phosphorus analysis. A correction for the weight of this salt was made. Iron determinations were carried out colorimetrically with *o*-phenanthrolene, using a modification of the method of Barkan and Walker[2]. The iron content of the hæmoglobin samples is presented in Table 1.

Assuming that the pigment in the destromatized hæmolysate is representative of the pigment inside the intact red cell, the results of these studies demonstrate that most, if not all, of the pigment in iron-deficient red cells has a normal iron content. It is apparent that the nearly identical iron content of the purified hæmoglobin solutions from normal and iron-deficient subjects cannot be due merely to failure of an incomplete hæmoglobin to crystallize, since there was very little difference in the iron content of the mother liquors from the first crystallizations of the rat hæmoglobin. Thus in iron deficiency a defect in the hæm synthesis appears to limit somehow the synthesis of globin.

Ernest Beutler
Argonne Cancer Research Hospital,
U.S. Atomic Energy Commission,
and
Department of Medicine,
University of Chicago,
Chicago, Illinois.
Dec. 19.

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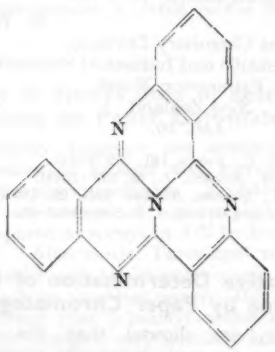
Carcinogenicity of Tricycloquinazoline

Tricycloquinazoline (I) has hitherto been prepared from indazole and copper powder[1,2], from *o*-aminobenzaldehyde and ammonium chloride[3] and from *o*-cyanoanilinium toluene-*p*-sulphonate[2]. We now find that it is readily formed in simple pyrolytic reactions. For example, methyl anthranilate was refluxed into a red-hot tube; a benzene solution of the resulting tar, after removal of its constituent acidic and basic fractions including methyl anthranilate, furnished 0·053 per cent of pure tricycloquinazoline by chromatography on alumina.



(I)

It was also produced, in the yields given, by the action of heat on the following compounds or mixtures: ammonium anthranilate (0·083 per cent), anthranilamide (0·73 per cent), anthraniloylanthranilamide (5 per cent), anthranilamide toluene-*p*-sulphonate (20–25 per cent), methyl anthranilate and ammonium benzenesulphonate (5–16 per cent), ammonium anthranilate and ammonium benzenesulphonate (0·11 per cent), anthranilic acid and ammonium benzenesulphonate (0·086 per cent), anthranilamide and ammonium benzenesulphonate (12 per cent). In each instance the product was authenticated by its melting-point and mixed melting-point, by its characteristic ultra-violet light absorption and by its R_F value.

Tricycloquinazoline can be sublimed unchanged at red heat at atmospheric pressure and is resistant to oxidation. This substance is a representative of a class of polyazapolycyclic hydrocarbons which have not previously been tested for carcinogenic properties, and so the activity has been examined following subcutaneous injection in the rat.

The compound was injected subcutaneously as a suspension in olive oil (10 mgm./ml.) into 30 Wistar

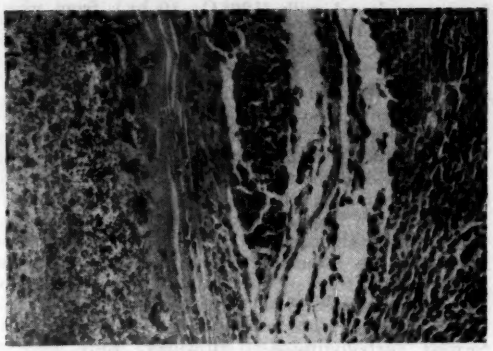


Fig. 1. Photomicrograph showing relation of neoplastic tissue to injected tricycloquinazoline. (× *c.* 90)

rats, 7–11 weeks old and equally divided with respect to sex. Because of the persistence of the material at the site of injection, rats received only four monthly injections, representing a total dosage of 40 mgm., and were then observed for 26 months.

The injected tricycloquinazoline proved to be extremely resistant to metabolism, and subcutaneous cysts containing the compound remained at the site of injection during the whole period of the experiment (22–26 months). In two experiments where attempts were made to recover tricycloquinazoline from injected rats after 22 months, in each case a total of 36 mgm. out of 40 mgm. injected was recovered from the local lesion in an unchanged form.

Tumours were observed in 27 per cent of treated rats during the period of the experiment and the tumour-induction period was relatively high. The first tumour appeared 14 months after the first injection, while the majority of the tumours developed between the sixteenth and twenty-fourth months. All these tumours were undifferentiated spindle-cell sarcomata which had arisen at the site of injection. Tricycloquinazoline causes a low-grade inflammatory reaction in the surrounding connective tissue, with ultimate formation of a cystic space surrounded by fibrous tissue. The inactivity of the compound is indicated by its demonstration by fluorescence microscopy as long as 26 months after injection. Histological studies enabled the close relation of the tumours to be seen (Fig. 1).

The latent period for tumour induction with tricycloquinazoline is considerably greater than that observed with polycyclic hydrocarbons[5]. This may be due to the extreme inertness of the compound, and it is interesting that similar latent periods have been observed for tumour induction by implants of synthetic polymers, which are also chemically unreactive[6].

Although it is not suggested that the carcinogenic activity of these various substances is due to a common mechanism, it would appear that, by virtue of its chemical inertness, the carcinogenic response produced by tricycloquinazoline more closely resembles that caused by plastics[6] and metals[7] and cannot be directly compared with the activity of the more readily metabolizable polycyclic hydrocarbons.

In view of the wide occurrence of methyl anthranilate in plants[4], and the fact that tricycloquinazoline can be readily prepared from it by the action of heat, the products of pyrolysis of plant material are now being examined for the presence of tricycloquinazoline. Further studies are also in progress to determine the carcinogenic activity of the compound administered by several routes in mice and rats.

R. W. Baldwin
K. Butler
F. C. Cooper
M. W. Partridge

The University,
Nottingham.

G. J. Cunningham

Royal College of Surgeons,
London.

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A Micro-electrophoretic Method

A MICRO-ELECTROPHORETIC technique has been developed which is sensitive, simple and rapid and does not require any elaborate apparatus. Very clear results can be obtained with samples of the order of fractions of a micro-litre with a protein content of 10 μgm. and even less. The technique is a combination of the membrane filter electrophoresis[1] and, in particular, of the small-scale technique[2], with the use of a very sensitive stain—'Nigrosin'[3]. All details of the membrane-filter electrophoresis apply as well to the micro-technique.

A horizontal electrophoretic tank for small-scale membrane-filter electrophoresis, with a bridge gap of 8 cm. is used. The buffer compartments run through the whole length of this tank, convection currents are considerably reduced and more efficient vapour saturation and cooling are provided.

The supporting medium consists of cellulose acetate-membrane filter strips (Oxo, Ltd., 16 Southwark Bridge Road, London) 10–12 cm. × 2·5 cm., with strip-holders lined with filter paper acting as wicks. Longer or wider strips can be used (and may be easier to handle). 120 V. applied for about two hours provides a pattern length of approximately 5–6 cm.

The sample is applied by means of a very fine calibrated capillary, micro-pipette or micro-syringe, about 2 cm. from the cathode end. The position of the sample is important and partly determines the pattern. The size of the sample can vary from 0·0001 ml. to about 0·008 ml. (0·1–8 μl.). Staining is carried out in very dilute 'Nigrosin' solution (0·005 per cent in 2 per cent acetic acid) for about one hour, the time being judged best by experience. The strip is washed in 2 per cent acetic acid, blotted and dried. 'Nigrosin' is more suitable for small samples. For samples containing more than 70 μgm. of protein conventional stains are to be preferred.

Patterns obtained with the method described are shown in Fig. 1. Clear separation patterns were still obtained on serum samples not exceeding 0·0001 ml. containing about 5–10 μgm. of protein. Two pre-albumin fractions could be clearly demonstrated in human serum (Fig. 1*b*). An important feature of this technique is that with small quantities of protein used some minor protein fractions become unmasked.

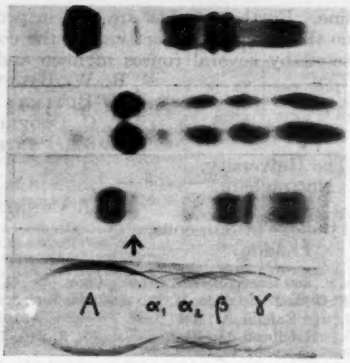

Fig. 1. Human serum, veronal buffer *p*H 8·6, *a*, *b*, *c*, 'Nigrosin' stain, *d*, azocarmin. Contact prints from uncleared strips. (*a*) 0·5 μl. 30 μgm. protein; (*b*) two pre-albumin fractions, 30 and 40 μgm. protein; (*c*) small post-albumin fraction (indicated by arrow) 10 μgm. protein; (*d*) micro-immuno-electrophoresis on 0·0025 ml. serum (180 μgm. protein)

A post-albumin fraction, for example, has been found in most of the human sera investigated (Fig. 1*c*).

The micro-technique described above can also be applied to immuno-electrophoresis (Fig. 1*d*). This is performed in a manner similar to that reported in a previous paper[4], taking into account the smaller size of the strip. The thickness of the agar layer should not exceed 1–2 mm., and the distance between the electrophoretic and antiserum strips is reduced to 3–5 mm., the width of the antiserum strip being about 1 mm.

J. Kohn

Pathology Department,
Queen Mary's (Roehampton) Hospital,
Roehampton,
London, S.W.15.
Jan. 16.

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Formation of Lactulose and Tagatose from Lactose in strongly heated Milk

In recent years it has been shown that reductones, furfurals and other related substances formed in heated milk are sugar fission products[1,2]. The mechanism by which lactose is degraded to these products has interested food chemists, but nothing is known of the transformation of carbohydrates in heated milk, in spite of considerable attention paid to them in normal cow's milk[3–5].

In order to confirm the secondary formation of carbohydrates in heated milk, the browning whey from autoclaved milk (120° C., 10 hr.), from which the curds had been removed, was chromatographed on Tōyō No. 50 filter paper by the ascending technique using the mixture *iso*propanol/pyridine/acetic acid/water (8 : 8 : 1 : 4)[6], and phenol saturated with water as solvent systems. The chromatogram was coloured by spraying with resorcine and aniline hydrogenphthalate reagents. The control sample was prepared from raw milk by a method essentially similar to that described by Sasaki and Taniguchi[5]. In the case of heated milk the chromatograms gave two new spots corresponding to tagatose and lactulose, and the intensity of the galactose spot was greater than that of raw milk. The lactose and glucose spots were less intense than in raw milk. These results are not sufficient to identify the newly formed carbohydrates. However, they indicate clearly that the same carbohydrates are formed secondarily from lactose in milk during heating in the autoclave.

The isolation and identification of these carbohydrates were carried out by Whistler and Durso's[7] and Samuelson and Sjoström's[8] methods. The sample from autoclaved milk (120° C., 8 hr.), desalted by passing it successively through columns of the ion exchange resin 'Amberlite-*IRA* 410' (OH^- form) and 'Amberlite-*IR* 120' (H^+ form), was poured into a column packed with a mixture of equal weights of 'Sagi' activated charcoal and 'Celite No. 535' filter aid. It was then developed with water followed by 5 per cent ethanol. These eluting fractions were passed separately through a column of 'Amberlite-*IRA* 410' (HSO_3^- form) and evaporated under reduced pressure. The crystalline carbohydrates isolated from the resultant syrups by methods given in the literature[9,10] were identified, by the determination of physical constants in conjunction with the results of the formation of characteristic derivatives, as *d*-tagatose and lactulose, from the water and ethanol fractions, respectively. These results are shown in Table 1. Galactose was identified in the water fraction as *p*-tolylhydrazone.

On the basis of these results, it seems reasonable to assume that the successive transformation of lactose to lactulose, galactose and tagatose in heated milk occurs not only in accordance with the Amadori rearrangement (as shown in previous papers[11]) but also *via* the de Bruyn and van Ekenstein reaction in the milk protein–lactose system. These findings are supported by the facts that tagatose was not formed while lactulose and galactose were, from *p*-tolyl–*iso*lactosylamine, the Amadori rearrangement product of *p*-tolyl–lactosylamine[12], under the conditions described above. Tagatose is doubtless the de Bruyn and van Ekenstein reaction product of galactose. Since lactulose has not been hitherto reported as a constituent of any natural product, while naturally occurring tagatose has recently been discovered in the gum

Table 1. Identification Data of Crystalline Ketoses eluted from Carbon Column

Sample	M.P. (° C.)	$[\alpha]_D^{20}$ (24 hr., in water)	Phenylosazone M.P. (° C.)	Phenylosazone Nitrogen (per cent) Found	Phenylosazone Nitrogen (per cent) Calculated	M.P. mixed with corresponding synthetic sugar
Carbohydrate obtained from water fraction	132–133	—	186	15·48	For $C_{18}H_{22}O_4N_4$ 15·64	With tagatose 132–133° C.
Carbohydrate obtained from 5 per cent ethanol fraction	156–157	−50·7	200	10·80	For $C_{24}H_{32}O_8N_4$ 10·89	With lactulose 156–157° C.
d-Tagatose	132–133	− 2·8	186	15·55	For $C_{18}H_{22}O_4N_4$ 15·64	—
Lactulose	156–157	−51·0	200	10·85	For $C_{24}H_{32}O_8N_4$ 10·89	—

exudate of *Sterculia setigera*[13], the presence of these ketoses in heated milk is of particular interest. These results may be applied to the transformation of the carbohydrates in evaporated milk[14] and other related dairy products.

A more detailed account of these experiments will be published elsewhere. I wish to thank Prof. T. Nakanishi for his guidance and encouragement in the course of this work.

SUSUMU ADACHI

Laboratory of Animal Products Technology,
Faculty of Agriculture.
Tohoku University,
Kita Roku Bancho, Sendai, Japan.
Dec. 7.

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Synthesis of Phlorin and other Phenolic Glucosides by Plant Tissues

ALTHOUGH phloroglucinol itself seldom occurs in plants it has been implicated in the biosynthesis of ring *A* of the flavonoid compounds[1,2]. In the course of a study of the metabolism of the glucoside, phloridzin, in *Malus*, leaf disks were fed phloroglucinol together with glucose labelled with carbon-14, and although phloridzin synthesis was unaffected a new compound was detected in chromatograms which has been tentatively identified as phlorin[3], the glucoside of phloroglucinol.

The methods used were as follows: 4 or 5 leaf disks, 1·2 cm. diam., obtained with a Ganong leaf punch, were infiltrated under vacuum with tap water, blotted and placed in 0·6 ml. of a solution of 600 μgm. phloroglucinol and 50 μgm. (1 μc.) labelled glucose contained in a small Petri dish. The dish was covered and placed in the light at room temperature. After 24 hr. the disks were removed, washed thoroughly with water and extracted for ½ hr. with boiling 80 per cent ethanol. The ethanolic extract was reduced in volume by means of an air jet at room temperature and applied, without further purification, to Whatman No. 1 paper for chromatography. The solvents were phenol saturated with water for the first direction and butanol/acetic acid/water (4 : 1 : 5 v/v) for the second.

Phlorin was detected on chromatograms by colour reactions with vanillin in concentrated hydrochloric acid[4], diazotized sulphanilic acid[5] and diazotized *p*-nitroaniline[6]. Radioactive phlorin was eluted from chromatograms with hot ethanol and hydrolysed with either 2 *N* hydrochloric acid or emulsin after removal of the ethanol. Chromatography of the hydrolysis products gave phloroglucinol and labelled glucose. Successful hydrolysis with emulsin showed that the compound was a β-glucoside. When phloroglucinol labelled with carbon-14 and inactive glucose were fed to disks, a radioactive spot, identical with the one obtained from phloroglucinol and labelled glucose, was obtained.

Table 1. DISTRIBUTION OF CARBON-14 IN 80 PER CENT ETHANOL-SOLUBLE FRACTION OF LEAF DISKS OR SEGMENTS FED LABELLED GLUCOSE AND PHLOROGLUCINOL

Leaf disks or segments	Per cent radioactivity of 80 per cent ethanol-soluble fraction				
	Phlorin	Glucose	Fructose	Sucrose	Glucitol
Adiantum macrophyllum	11	<1	<1	82	—
Pinus resinosa	62	1	—	37	—
Briza media	27	1	—	64	—
Pelargonium hortorum	76	3	—	1	—
Kalanchoë punctata	3	46	42	9	—
*Gerbera jamesonii**	82	1	—	5	—
Malus sylvestris	29	1	—	5	64

* When leaf disks of this plant were fed phloroglucinol labelled with carbon-14 and inactive glucose under the same conditions, 90 per cent of the radioactivity of the 80 per cent ethanol-soluble fraction was in phlorin.

That this ability to synthesize phlorin is not peculiar to apple-leaf tissue is shown in Table 1. It was found that phlorin synthesis occurred not only in the margins but also in the central regions of the disks, and, moreover, light was not necessary to effect this synthesis. Except for *Adiantum*, free phloroglucinol was not detected in any of the other plant extracts. When starved leaves, that is, leaves on plants maintained in the dark for 48 hr., of *Pelargonium hortorum* or *Gerbera jamesonii* were used, however, phlorin synthesis was found to decrease, and free phloroglucinol accumulated in the tissues. This occurred even in the presence of exogenous glucose.

Although other sugars such as fructose, galactose, rhamnose, arabinose or xylose were substituted for glucose, no glycoside other than phlorin was detected on chromatograms.

Other phenols, including orcinol, resorcinol, hydroquinone, salicyl alcohol, *p*-hydroxybenzaldehyde and vanillin were similarly fed with labelled glucose to leaf disks of *Malus* and *Gerbera*. Each phenol gave rise to what appeared to be the respective glucoside regardless of whether it occurred naturally in the plant, and appeared as a distinct, intensely radioactive spot on the chromatogram.

So far as we know, this ready synthesis of phenolic glucosides by plant tissues has not been described, although Dr. A. E. Schwarting of the University of Connecticut (private communication) has found that salicyl alcohol, when fed to twigs of *Salix nigra*, gives increased amounts of salicin in the bark.

Phenols have been prominent in theories of disease resistance in plants[7–13]. Disease resistance has been attributed to either an increased production of phenols in healthy tissue adjacent to regions invaded by the parasite or to a naturally high phenol content of the host. The increase in free phenols often observed in healthy tissue of parasitized plants may be due to an inability to 'detoxify' phenols liberated by enzymic hydrolysis of the natural glycosides of the plant. The inability to 'detoxify' phenols may be caused by glucose depletion as a result of metabolic activity of the pathogen. These phenols permeate both host and parasite tissues. The extent to

which pathogenic bacteria and fungi either metabolize or 'detoxify' phenols is not clear. Recently, certain phenolic acids, hitherto considered to be characteristic of higher plants, have been detected in a bound form (possibly as glycosides) in uredospores of wheat stem rust[14]. It is possible that these compounds are formed in the fungus through glycosidation subsequent to the absorption of phenolic acids. If phenols are to be implicated in theories of disease resistance it would appear that studies of the metabolism of these compounds in both host and parasite may be of the greatest importance.

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A. Hutchinson
Chitra Roy
G. H. N. Towers

Botany Department,
McGill University,
Montreal 2.

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Effects of Clipping and Spraying on Various Homœothermic Responses of Rabbits at an Elevated Environmental Temperature

The relative effects of clipping and spraying on evaporative and non-evaporative cooling have been measured on rabbits at an elevated air- and body-temperature. Under such conditions most of the heat loss is by evaporative cooling.

Rabbits were maintained in individual cages in the alley of the University of Missouri Climatic Laboratory at 103° F. and 15 per cent relative humidity, in an air velocity of 50 ft./min. Drinking water and rabbit feed (pellets) were available *ad libitum* from hopper-type feeders and waterers. However, at 103° F. air-temperature the rabbits consumed no food, except occasionally after spraying.

Before exposure to an air-temperature of 103° F., four of the eight experimental rabbits were sheared as closely as possible with small animal clippers. After being at 103° F. for 4–5 hr., the animals were sprayed manually with an atomizer (15 lb./in.^{2} air pressure), using water at room-temperature (103° F.). Each rabbit was sprayed for 5 min. using only enough water to produce a fine mist. Conduction between the skin and (spray) water was thus not likely to be an appreciable factor in loss of heat.

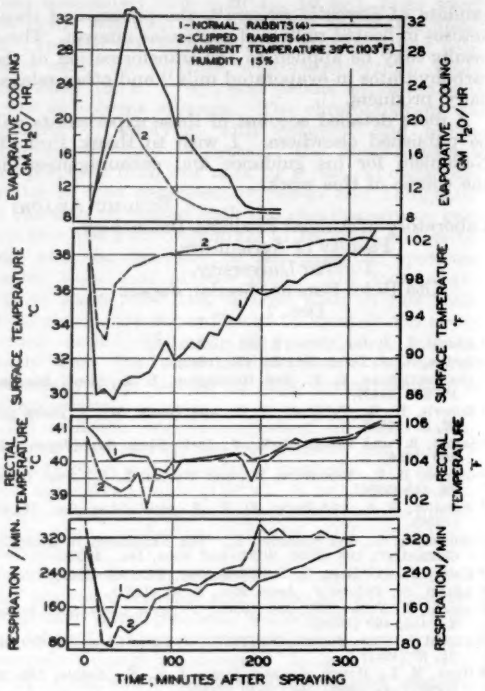

Fig. 1. Influence of spraying on various reactions of normal and clipped rabbits

Respiration-rates were measured by counting the flank movements of the animals in the cages. Each point in Fig. 1 represents an average of three determinations. Surface temperatures were taken by the Hardy dermal radiometer. Evaporative cooling (insensible weight-loss) was measured by weighing the animals on a large, sensitive, 'chainomatic' scale placed in the Climatic Laboratory. Rectal temperatures were taken by clinical thermometer.

Merely clipping the rabbits at 103° F. showed no striking effect on the homœothermic responses of the animals, although it was beneficial. Average values for the clipped rabbits at that temperature were: respiration-rate, 335 per min.; surface temperature, 38·8° C.; rectal temperature, 40·68° C.; and evaporative cooling, 9·26 gm. water/hr.; corresponding values for the normal rabbits were: respiration-rate, 290 per min.; surface temperature, 37·5° C.; rectal temperature, 41·1° C.; and evaporative cooling, 8·66 gm. water/hr. Clipping exposed the warm skin directly to the ambient air, resulting in a slightly greater rate of evaporation, a greater rate of non-evaporative cooling, and a lower rectal temperature. Respiration-rates were higher in the clipped rabbits. All these measurements indicate some benefit from clipping at high temperature.

However, there were striking differences in the responses of clipped and normal rabbits to spraying. Fig. 1 (top) shows the evaporative cooling of New Zealand white clipped and unclipped rabbits after spraying. Rabbits with normal coats showed a great increase in evaporative cooling in contrast to the clipped animals (cf. curves 1 and 2 in Fig. 1), amounting to approximately fourfold 50 min. after spraying for the normal rabbits, and only twofold for the clipped animals. These increased rates of

evaporative cooling after spraying persisted for about 125 min. for clipped rabbits and 175 min. for normal rabbits. Thereafter both curves return to their initial level for the environmental temperature of 103° F.

The surface temperature (Fig. 1, second section) was lower for the normal-coated rabbits, for about 275 min. after spraying, due to the greater surface evaporative moisture. There was, presumably, less loss from non-evaporative cooling.

The lower rectal temperature of the clipped rabbits reflects a greater immediate physiological response to spraying than in normal rabbits (Fig. 1, third section) for the first 75 min. after spraying. Thereafter less difference between the rectal temperature of clipped and normal rabbits was noted. A greater immediate benefit of the clipped rabbits to spraying is also shown in the respiration-rate (Fig. 1, bottom section), which, however, increases considerably after about 100 min.

Thus the physiological and physical responses of the clipped and normal rabbits to spraying are quite different. The more sensitive respiration-rate of the normal rabbits 145–300 min. after spraying demonstrates the value of wet fur, the action of which may be compared to that of a moist sponge-rubber blanket[1]. Following spraying, the rectal temperature and respiration-rate declined rapidly, though only temporarily (Fig. 1), particularly in clipped rabbits. The major cooling factor responsible for the decline in this case was undoubtedly the increase in evaporation, since in both clipped and normal rabbits their surface temperatures (radiometer reading) were below that of the environment.

HAROLD D. JOHNSON
CHU SHAN CHENG
A. C. RAGSDALE

Department of Dairy Husbandry,
College of Agriculture,
University of Missouri,
Columbia, Missouri.
Oct. 10.

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Validity of Motokawa's Technique for investigating Retinal Function

IN the past decade, Motokawa and co-workers have published in about sixty papers the results of a new technique for investigating retinal function. Gebhard[1] has published a review of this subject. Michaels[2] claims to have confirmed Motokawa's observations. Recently, Riggs *et al.*[3], and ourselves[4], working independently, have failed to confirm the most important of Motokawa's results. We believe that there are serious flaws in Motokawa's and Michaels' technique and that their experimental results may be artefactual.

Motokawa's technique is based on the curve of the change of threshold for electrical stimulation of the human eye after various types of light stimulation. The threshold is said to be minimal 1 sec. after red light, 2 sec. after white and green and 3 sec. after blue.

Both Motokawa and Michaels use very highly trained subjects and rather unusual psychophysical methods. Motokawa starts with a stimulus well above threshold and reduces the intensity in fairly large (10 per cent) steps until the suspected threshold is approached; the step size is then reduced (to about 1 per cent) and, when requested by the subject, a zero stimulus may be given for comparison. Michaels gives stimuli which are alternately above and below the threshold and approaches the threshold by a method of "successive approximations". Motokawa's method will certainly contain a source of error due to the greatly increased probability of finding a threshold when the large steps are changed to small ones. Michaels' results would be affected by the range of high and low intensities selected by the experimenter and by any sequential response habits, such as alternation, of the subject. The use of very highly trained subjects could set up unconscious subject-experimenter interaction, since the long training period seems to continue until the subjects give the 'correct' results. The consistency of threshold determinations by Motokawa's subjects is much better than that found for any other visual threshold. This in itself induces a feeling of uneasiness about the methods used.

Riggs *et al.* used a few trained subjects and attempted to reproduce Motokawa's technique as closely as possible. However, they had a rigidly defined threshold criterion, and they always reduced the intensity of the stimulus by a constant fraction. This meant that they gave, in a single run, rather more stimuli well above threshold than did Motokawa. Kohata *et al.*[5] have now claimed that the relatively intense stimuli used at the beginning of the run produce an after-effect which obscures the fall in the phosphene threshold and increases the variability of the measurement.

In our own experiments, we reduced the intensity of the stimulus by equal steps on a linear scale, and each intensity was presented only once. The sequence was stopped after the subject had failed to see three consecutive stimuli. A variety of threshold criteria was applied to our data (including taking the lowest positive response), but all gave essentially the same result. We used both naïve and experienced subjects and intervals between the light flash and the electrical stimulus of 1, 2, 3, 4 and 9 sec. In almost all our subjects the threshold was lowest in the 1-sec. condition and rose in an approximately exponential manner for the longer intervals no matter what the colour of the light flash, and even if the light were replaced by a bell. A very similar result was obtained also for an auditory difference threshold following a brief flash of light. We interpreted this effect as being due to the initial light or bell acting as a warning to the subject when to expect the stimulus. Following this interpretation we found that the phosphene threshold was completely unaffected by a preceding flash of light, provided a bell rang at a constant interval (1 sec.) before the electrical stimulus. This condition provided our most stringent test of Motokawa's basic observation, since it excluded the warning effect.

We have met the objection of Kohata *et al.* by repeating this experiment, but approaching the threshold from below, to exclude the possible after-effects of strong stimuli[4]. We did not find in either case any effect of the light on the electrical threshold, nor was there any marked change in the variability of the threshold measurements. The results of these experiments are shown in Table 1. It may be that only the lowest of the multiple thresholds which Motokawa claims to have observed will give the Motokawa phenomenon, and that Riggs *et al.* and ourselves may have observed higher and inappropriate thresholds. However, neither we nor Riggs *et al.* have been able to record multiple thresholds even in highly trained subjects.

Table 1

Interval between light and electrical stimulus (sec.)	1	2	3	4	9
Mean threshold when approached from above (log μamp.)	2·0535	2·0425	2·0629	2·0633	2·0728
Mean standard deviation	0·0628	0·0688	0·0641	0·0547	0·0737
Mean threshold when approached from below	2·1223	2·1422	2·1111	2·1255	2·1564
Mean standard deviation	0·0488	0·0299	0·0473	0·0682	0·0407

The number of subjects in the two conditions were 10 and 6 respectively. Each subject gave four estimates of each threshold. Unfortunately they were largely different subjects so that inter-subject variability cannot be ruled out in comparing the two sets of figures.

The most stringent test of the multiple threshold hypothesis is obviously to present a range of intensities near the threshold in random order (method of constant stimuli) and to see if the probability of seeing changes monotonically with the intensity or in a series of steps. Riggs *et al.*[3] and Howarth[4] have done this each using just one experienced subject and find that the probability of seeing changes in a 'normal' manner is 25–75 per cent between intensities of 110–145 μamp. There is no evidence of discontinuities in these curves.

C. I. Howarth
M. Treisman

Institute of Experimental Psychology,
University of Oxford.
Nov. 28.

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Passage of Phosphate through the Human Tooth

The first experiments using isotopes on teeth appear to be those of Hevesy *et al.*[1]. They administered phosphorus-32 to two human subjects, and to cats and rats, then found it in both dentine and enamel, attributing this finding to ion exchange. Later work with other isotopes (those of hydrogen, iodine, sodium, calcium, sulphur, carbon and potassium) showed that labelled ions may pass into tooth structure, more readily into dentine than into enamel. However, apart from Hevesy *et al.*, such work seems to have been restricted to animals (see ref. 2 for summary).

The present investigation describes work on the movement of phosphate through human teeth. The teeth have been tested as quickly as possible after removal from the mouth. The pulps of such teeth are known to respire[5]: $Q_{O_2} = 0{\cdot}31$, $RQ = 0{\cdot}93$.

Sound bicuspid teeth from subjects aged between 9 and 12 years were used, and, within 10–15 min. of removal, preparation of the teeth was completed. Crowns and roots were separated at the gum-line, cutting across the central root-canal; crowns were used. The bulk of the pulp was then removed from

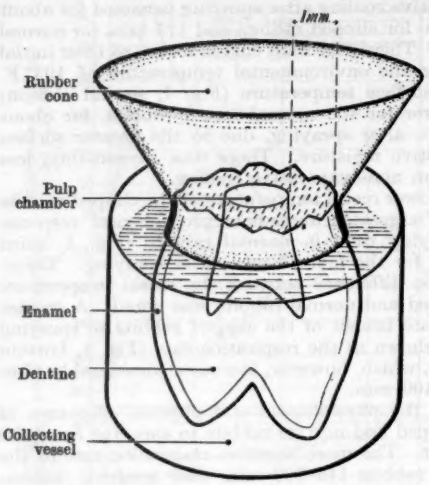

Fig. 1. Test of passage of phosphorus-32 through tooth. The labelled phosphate is added to the pulp chamber by a fine pipette. Later the tooth-crown is removed when the collected fluid is examined for radioactivity

the pulp chamber leaving it lined by odontoblasts. Four methods of examination (the first three microscopical) were used for confirming the presence of odontoblasts *in situ* in such preparations: (1) de-pulped teeth were examined after staining with methylene blue, when odontoblasts have been seen *in situ*; (2) scrapings from pulp-chamber walls, spread on glass slides and stained with methylene blue, show odontoblasts; (3) de-pulped teeth were sectioned by microtome (after decalcification); examination of stained sections shows a ring of odontoblasts *in situ* lining the pulp-chamber; (4) de-pulped teeth were decalcified and examined by electron micrograph, when odontoblasts are seen *in situ*[6].

On being de-pulped, a tooth crown intended for test was at once filled with Krebs–Ringer solution saturated with 95 per cent oxygen/5 per cent carbon dioxide at 37°. The tooth crown was then clamped in a rack, and by means of a pinion lowered into a small vessel of 1·6 ml. Krebs–Ringer at 37° so that the crown is submerged to the cut and exposed dentinal aspect, while this remains above the fluid level (Fig. 1). Splash is avoided by fitting a thin rubber cone with a 1-mm. hole in the apex,

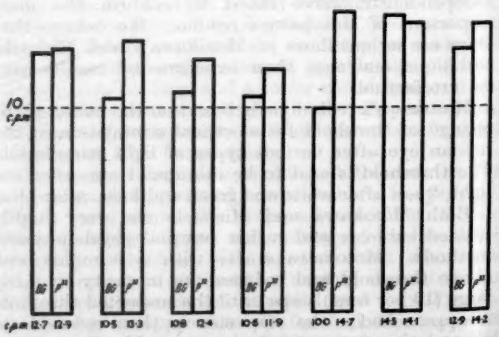

Fig. 2. Collected labelled phosphate and background compared on passage of labelled phosphate through each of seven teeth

Table 1. Results of Seven Tests

Experiment No.	$\bar{X}$ c.p.m. of collected phosphate	Background (c.p.m.)
1	12·9	12·7
2	13·3	10·5
3	12·4	10·8
4	11·9	10·6
5	14·7	10·0
6	14·1	14·5
7	14·2	12·9

around the tooth; the cone extends about 1·5 cm. upwards. Creep is avoided by tying a thin, strong cotton thread tightly around and close to the base of the rubber cone.

Inorganic phosphate labelled with phosphorus-32 was now added to the Krebs–Ringer in the pulp-chamber. Every 5 min. the tooth-crown was racked up and a fresh vessel of Krebs–Ringer brought underneath, when the tooth-crown was lowered into it. The contents of the pulp-chamber were changed for fresh Krebs–Ringer about every 15 min. A test series usually comprises 20–24 such vessels, each of 1·6 ml. Krebs–Ringer, which collect any labelled phosphate that passes through the tooth. A test takes in all about 2 hr. The collections of fluid were dried and their radioactivity determined. Background counts were made before, during and after tests; all counting periods were of 5 min. duration.

Passage of phosphorus-32 is seen to occur through human teeth when introduced in inorganic form, and results are shown in Table 1 and Fig. 2. In each case the c.p.m. shown is the mean of all the counts made on the one tooth collection. Actual counts of phosphorus-32 range from 9·4 to 62·6 c.p.m., while backgrounds ranged from 8·8 to 17·2 c.p.m. Combining the tests[5] gave a χ^2 of 51·7 on 14 d.f., which is highly significant, $P < 0{\cdot}001$.

In one test, phosphorus-32 was collected for the usual 5-min. period, c.p.m. being 14·2; a 30-min. collection was then made, which gave a mean of 15·4 c.p.m., reasonably close to the mean, and which may indicate a steady rate of passage. In each experiment, activity was determined of each of the vessels (about twenty) containing phosphorus-32 which had passed through the tooth; the mean of each of these series is shown in Table 1 and Fig. 2.

Thus passage of phosphorus-32 through odontoblasts, dentine and enamel occurred in these tests on human teeth which are believed to be alive. However, such passage is limited in extent, any part played by the odontoblasts being unknown, though the use of suitable fluid media is aimed at keeping the cells viable. There are here two supporting observations to be kept in mind: (1) under suitable conditions tooth-pulp respires for some hours after removal of the tooth from the body; (2) de-pulping of a tooth leaves odontoblasts *in situ*, each cell held in place by the long cell-process extending through the dentine as far as the enamel.

From the present work, enamel appears more permeable than has hitherto been supposed, though little comparable work has been found in the literature. This permeability may well be related to the inhomogeneity of enamel. Saunders *et al.*[6], and Symons describe two kinds of ameloblasts in developing enamel. During development, one kind of cell (a minority) shows special staining reactions and also a higher ribonuclease activity[7]. While completed enamel is devoid of cells, residual pathways of enzymic activity greater than the bulk of enamel could facilitate passage. Histochemical examination of enamel may yet provide evidence here. Autoradiography of human teeth through which phosphorus-32 has passed presents difficulties not completely overcome so far.

My thanks are due to Prof. R. Douglas Wright, Sir Arthur Amies and the University of Melbourne Research Fund.

P. Pincus

Department of Physiology,
University of Melbourne.
Dec. 12.

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Blocking Effect of Morphine on the Secretion of Thyroid-stimulating Hormone in Rats

The ability of morphine to inhibit certain pituitary functions has been established. The administration of morphine on the day of pro-œstrus resulted in the suppression of ovulation[1]. It is also possible by pre-treatment with morphine to prevent the secretion of adrenocorticotrophic hormone which follows an injection of adrenaline or other stress[2]. Sawyer *et al.*[3] have observed that morphine in doses sufficient to block the activation of the pituitary depressed the electrical activity of the central nervous system and also of the hypothalamic centres. The present experiment was designed to ascertain whether morphine is capable of blocking the secretion of the thyroid-stimulating hormone of the pituitary also.

Male rats of the Wistar strain, weight 140 gm., were used in this investigation. The right lobe of the thyroid and half the isthmus were removed under ether narcosis and the animals were divided into two groups. The control group received a daily dose of 0·1 ml. saline solution subcutaneously, while the other group received morphine in a daily dose of 2 mgm. per rat. After 14 days the rats were injected intraperitoneally with 25 μc. of iodine-131 and 24 hr. later were exsanguinated from the abdominal aorta. The following parameters were evaluated for the remainder of the thyroid: weight of the thyroid lobe, epithelial cell height, uptake of iodine-131 in the thyroid and percentage of iodine-131 which

Table 1

Group	Control (8 rats)		Treated (17 rats)	
Partial thyroidectomy	Before	After	Before	After
Weight of one lobe of thyroid (mgm./100 gm.)	4·05* ± 0·25	4·76 ± 0·40	4·32 ± 0·17	4·46 ± 0·16
Epithelial cell height (μ)	9·74 ± 0·32	11·27† ± 0·46	9·63 ± 0·26	9·27 ± 0·64
Thyroidal uptake of iodine-131 (per cent/100 mgm.)		61·24 ± 8·04		33·02‡ ± 5·53
Protein bound iodine-131 (per cent)		0·1313 ± 0·0114		0·0665‡ ± 0·0088

* Mean ± standard error. † $P < 0{\cdot}02$. ‡ $P < 0{\cdot}01$.

became bound during 24 hr. to proteins in serum.

The results are summarized in Table 1. No significant change was observed in the weight of the thyroid during the period. The average epithelial cell height in the control group, however, was significantly increased compared with the pre-operation values. In the animals treated with morphine there was no histological activation, as values before and after the operation remained unchanged. The thyroidal uptake of iodine-131 in the treated group was lower than in the controls. The difference is statistically significant ($P<0{\cdot}01$). The ability of the thyroid to bind iodine-131 to proteins was similarly lowered. The levels of protein-bound iodine-131 in serum were significantly lower than in the controls ($P < 0{\cdot}01$).

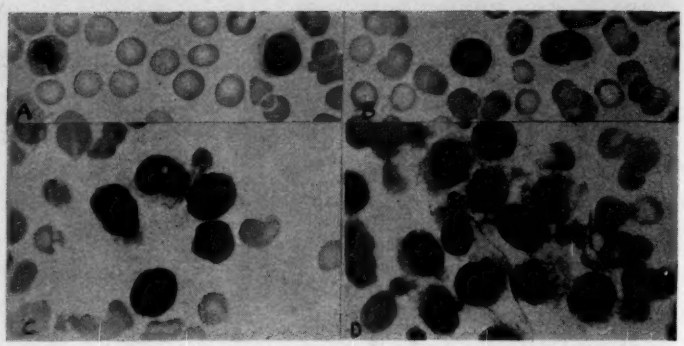

Fig. 1. *A*, Blood: a normoblast and abnormal lymphocyte with fringed cytoplasm *B*, blood: an abnormal lymphocyte with fringed cytoplasm and an ordinary lymphocyte *C*, bone marrow: abnormal lymphocytes with varying amounts of fringed cytoplasm *D*, spleen puncture: abnormal lymphocytes with fringed cytoplasm

The increased epithelial cell height in the control group suggests that the removal of half the thyroid tissue itself stimulates increased secretion of thyroid stimulating hormone. The administration of morphine prevented the morphological activation and markedly inhibited the thyroidal uptake of iodine-131 as well as the formation of hormonal iodine. Since all these functions of the thyroid are regulated by the thyroid stimulating hormone, it may be supposed that morphine is also able to block the secretion of the latter.

Milan Sámel

Slovak Academy of Sciences,
Endocrinological Institute,
Bratislava.
Oct. 10.

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An Abnormal Type of Lymphocyte

During the past four years I have noticed in normally coloured smears of blood, bone marrow and spleen puncture from 9 cases of hepatosplenomegaly with strongly marked granulocytopenia and lymphocytosis, a considerable number of cells closely resembling lymphocytes but showing several special features. I have never observed this type of lymphocytes in lymphatic leukosis, which I have studied for many years from the cytological point of view, nor in any other disorder of the lymphatic system.

These cells prevailed in smears of bone marrow. In smears of spleen puncture there was a great mass of cells consisting almost exclusively of this type of lymphocytes.

Fig. 1 shows these cells in blood, bone marrow and spleen puncture. Their size corresponds to the size of a large lymphocyte. The nucleus, which is generally larger than the nucleus of an ordinary lymphocyte and usually central, is round, oblong or kidney-shaped. Sometimes there are two round or oblong nuclei partly covering each other. The nuclear membrane is sharply outlined. The chromatin is delicate and of a fine network, clearly differing from the compact masses of chromatin in normal lymphocytes. Sometimes, however, there are small chromatin blocks divided from each other by clear zones similar to the nuclear pattern of an ordinary lymphocyte. Sometimes there is a barely perceptible nucleolus corresponding in character and location to the nucleolus of a rather young lymphocyte. The amount of cytoplasm varies, it is bluish in colour, and meshy, usually fringed on the whole periphery. Sometimes, however, the amount is much reduced, as if used up, with only small shredded particles conserved around the nucleus. Occasionally azurophil granules closely resembling those of an ordinary lymphocyte can be seen.

At first I was not sure whether these cells, which show a negative peroxidase reaction and stain only faintly red with methyl green and pyronin, belonged to the series of lymphocytes or not. The phase-contrast microscope, however, revealed the same character and arrangement of mitochondria as in a mature lymphocyte.

In the first five cases, the clinical course was acute. All five findings at autopsy disclosed a rare type of tuberculosis known as "sepsis tuberculosa gravissima[1]", "areaktive generalisierte Tuberkulose[2]", or "septicémie tuberculeuse foudroyante maligne[3]". The remaining four patients are still under clinical control. The clinical course is acute in one case, subacute in one case, and chronic in the other two.

As a routine designation in the hæmograms I have called these lymphocytes 'Dozet cells', after the surname of the first patient in whom I noticed them.

So far I do not know whether this lymphocyte is characteristic of "sepsis tuberculosa gravissima". This question remains to be answered by further observation. Yet it is of interest to direct attention to this cellular element not only from the morphological point of view but also as a diagnostic difference with respect to lymphatic leukosis.

B. Brausil

Hæmatology Laboratory,
Department of Medicine,
University of Zagreb,
Yugoslavia.
Dec. 18.

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Significance of the Adrenal Glands during the Post-natal Development of Thermoregulation in the Rat

THE range of environmental temperature within which rats maintain a stable body temperature increases with age[1-3]. It is therefore possible that the lack of effect of adrenalectomy on thermoregulation before the sixteenth day after birth[4-6] is the result of the relatively very low environmental temperature used (5° C.) at which thermoregulation fails in very young rats. In order to test this hypothesis rats were adrenalectomized under cold anæsthesia on the fourth day after birth. One, two and three days after operation their power of thermoregulation was tested as follows: rectal temperature was taken with a thermocouple immediately before the experiment after removal of infant rats from the nest. The animals were then placed in a Bargeton–Krumm–Heller[3] respirometer and oxygen consumption was measured at an environmental temperature of 35° C. for 30 min. Then the external temperature was changed to 30° C. at which readings were continued for a further 60 min. Finally, the rectal temperature was again taken.

It was found that rectal temperatures of operated animals always dropped to significantly lower values than those of sham operated controls. Oxygen consumption always rose when the environmental temperature was lowered to 30° C. If cortisone was administered for three days between the fourth and seventh day after birth to adrenalectomized rats via the intramuscular route (0·5 mgm./ml. saline; 1 ml./100 gm. body-weight) the effect of adrenalectomy was nullified. Rectal temperatures no longer decrease to such an extent at 30° C. external temperature and oxygen consumption at 35° C. is increased. If control animals are treated in the same way oxygen consumption is increased significantly and rectal temperatures do not fall at all.

Thus on the third day after adrenalectomy the rectal temperature drops from 33·5 ± 0·481° to 31·9 ± 0·298°, while in similarly treated animals receiving cortisone the final temperature is 33·3 ± 0·466°. In control animals receiving cortisone rectal temperature remains at 34·6 ± 0·274°. Oxygen consumption in normal infant rats after treatment with cortisone is raised from 41 ± 3·33 to 60 ± 1·41 ml./100 gm./10 min. at 35° C. surrounding temperature and to even higher values at 30° C.

The above results show that the adrenal glands participate in the maintenance of body temperature in five-day-old rats. On the other hand, they evidently are not indispensable for the increase in oxygen consumption which occurs when the external temperature is changed from 35° to 30° C. The negative results obtained by previous workers[4-6] are probably due to the low environmental temperature used.

P. HAHN
O. KOLDOVSKÝ

Institute of Physiology,
Czechoslovak Academy of Sciences,
Prague 6.

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'Gastric Para-Mucus'

WHEN an aqueous solution of either iodoacetamide or N-ethyl maleimide, in suitable concentration, is applied topically to the mucosa of a canine Heidenhain pouch half an hour before the injection of histamine, the hydrochloric acid secretory activity of the pouch is inhibited completely, though reversibly[1]. This inhibitory action may be presumed to involve a competitive inhibition of one or more enzymes within the parietal cells, though Davenport's assumption[2] that these enzymes contain sulphydryl still awaits confirmation. Instead of the water-like solution of hydrochloric acid ordinarily obtained in response to histamine stimulation, the pouch now pours out a mucinous cell-free liquid of *p*H greater than 7, and sometimes even as high as 8. Flow of this viscous liquid continues for many hours after drainage of the iodoacetamide or N-ethyl maleimide solution from the pouch, whereas the acid-secretory response to subcutaneously administered histamine usually lasts for no more than 1–1½ hr. Further evidence that this mucinous material bears no relation to the parietal secretion is given by the fact that the same response is obtained when the instillation of iodoacetamide or N-ethyl maleimide into the pouch is not followed by the subcutaneous administration of histamine.

This viscous fluid has the pale yellow or tawny appearance of blood plasma, even when it contains no visible blood, but its acid-neutralizing power, and its concentrations (after hydrolysis) of total solids, reducing substance, hexosamine, and total nitrogen are generally less than those for dog serum. Rather, these divers characteristics, together with its viscosity, suggest that we are dealing here with some kind of a mucinous secretion—different from the true gastric mucus obtained in response to topically applied acetylcholine[3], and certainly bearing no resemblance to the cellular variety of mucus formed in response to treatment of the pouch mucosa with eugenol[4]. It may be a modified form of mucus secretion proper, elaborated by the columnar epithelium of the mucosa, or it may be the hypothetical mucoid secretion of earlier investigators (sometimes identified with the dilution secretion or *Verdünnungssekretion*[5]); but as yet there is no sound evidence to support either such physiological characterization of this material. In view of this lack, and until such time as a definitive statement of its origin is possible, I propose to designate this fluid gastric 'para-mucus', because the Greek prefix (παρα) expresses (according to the Oxford English Dictionary) "faulty, irregular, improper, alteration, simulation, etc.".

About ten years ago, we reported from this laboratory that a 1 per cent aqueous suspension of mustard oil (allyl isothiocyanate) applied topically to the gastric mucosa, is a very poor mucigogue by comparison with irritants like an emulsion of clove oil, distilled water saturated with ethyl ether, etc., as evidenced by a persistently low viscosity, and probable small mucin content shown in metachromatically stained smears. Chemical analyses were not performed at that time. On the contrary, "the high volume, rate, and duration of secretion; the resemblance in colour of early specimens to blood plasma; the almost invariable appearance of fresh blood, increasing in intensity with progress of the experiment—all these, together with the observations on viscosity, opacity, etc., support the view that the 'secretion' is a transudate or exudate"[6].

It seems altogether possible that this material induced by mustard oil is essentially of the same nature as the gastric para-mucus evoked by iodoacetamide and N-ethyl maleimide, the presence of blood in the former being a result only of the great 'irritant' character of the allyl isothiocyanate in 1 per cent concentration. A fluid of similar appearance has been obtained from gastric pouches also following the topical application of 'Phenergan' (4 mgm./ml.) and, occasionally, 'Nupercaine'[7]. Whether these various mucinous fluids are identical with para-mucus obtained after the mucosa has been treated with iodoacetamide and N-ethyl maleimide can be decided only by chemical analysis. The results of electrophoretic, chromatographic and other chemical studies will be reported shortly.

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Franklin Hollander

Gastroenterology Research Laboratory,
Mount Sinai Hospital,
New York 29, New York.

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Distribution of Norepinephrine in the Heart

The concentrations of norepinephrine found by various investigators in the hearts of different animal species are summarized in the 1956 monograph by von Euler[1]. These values range from 0·3 to 1·0 μgm. per gm. of heart tissue.

Recent reports, however, indicate that the heart norepinephrine-levels may be even higher than those found in the earlier experiments. Measurement of the levels in rabbit heart by a simple and sensitive spectrofluorometric method[2] gave an average value of 2 μgm. per gm., a considerably higher level than those reported earlier, but agreeing with the levels reported recently by Bertler, Carlsson and Rosengren[3]. Raab and Gigee[4] and von Euler[5] have reported levels of about 0·2 μgm. per gm. in dog heart. Levels in dog heart as measured by the spectrofluorometric technique[3], however, showed a concentration of about 2 μgm. per gm.

The high concentration of norepinephrine in heart brings up the question of its function in this organ. It was thought that study of the distribution of the amine in various parts of the heart might shed light on this question, since, to our knowledge, no distribution studies have been carried out on this organ.

Dogs of both sexes were killed by intravenous injection of pentobarbitone or by exsanguination. The hearts were removed immediately, dissected, and the various parts analysed for norepinephrine and epinephrine by the spectrofluorometric method described elsewhere[3].

Little epinephrine was found in any portion of the heart, the levels being less than 10 per cent of the norepinephrine concentration. Norepinephrine-levels are shown in Table 1. The average concentration in the atria was 2·7 μgm. per gm. It is noteworthy that the concentration in the portion of the right atrium which includes the sinoauricular node was not significantly different from that in other atrial samples. The level throughout the ventricular muscle was quite uniform—about 1·5 μgm. per gm. The endocardium may be devoid of norepinephrine since the valves, which are endocardial structures, lack norepinephrine. Furthermore, removal of endocardium and epicardium from ventricular samples did not significantly alter the levels found.

Table 1. Distribution of Norepinephrine in Normal Dog Heart

Part of heart	No. of hearts	μgm. per gm. Mean value of norepinephrine concentration	Standard error of mean	Range of values
Atria				
Atria, pooled	6	3·1	± 0·3	1·9–3·6
Right auricular appendage	4	2·4	± 0·4	1·9–3·5
Right atrium without appendage	2	2·7	± 1·0	1·7,3·7
Portion containing sinoauricular node	4	2·3	± 0·5	0·8–3·1
Left auricular appendage	4	2·8	± 0·3	2·2–3·7
Left atrium without appendage	2	2·3	± 0·9	1·4,3·2
	22 determinations			
(Mean of 22 determinations is 2·7 μgm. per gm.)				
Ventricles				
Apex of heart	11	1·3	± 0·2	0·7–2·9
Right ventricle	4	1·4	± 0·3	0·8–2·1
Right papillary muscles	4	1·5	± 0·3	0·7–2·2
Portion containing atrioventricular node	4	1·4	± 0·1	1·1–1·6
Left ventricle	4	1·3	± 0·1	1·1–1·6
Left ventricle, epicardium and endocardium removed	2	1·8	± 0·6	1·2,2·4
Left papillary muscles	4	1·6	± 0·4	0·9–2·8
Septum	10	1·6	± 0·2	0·9–3·4
	43 determinations			
(Mean of 43 determinations is 1·5 μgm. per gm.)				
Valves of heart				
Semilunar valves	2	0	0	0
Mitral valve	2	0	0	0
Tricuspid valve	2	0·1	±0·1	0, 0·2
Major blood vessels				
Aortic arch	4	1·3	± 0·2	0·9–1·7
Pulmonary artery	3	1·1	± 0·3	0·8–1·6
Superior vena cava	1	0	—	0

The average norepinephrine concentration in the atria was significantly higher ($P < 0.001$) than in ventricles, the ratio being 1·8 : 1.

The concentrations of norepinephrine in the aortic arch and in the pulmonary artery were almost as high as in ventricular muscle, while the superior vena cava was apparently devoid of norepinephrine.

Since sympathetic denervation has been shown to reduce markedly the norepinephrine content of the heart[1], von Euler and other investigators concluded that the norepinephrine in the heart is associated with adrenergic nerve endings. Since stimulation of sympathetic nerves affects the heart-rate profoundly, it is perhaps not surprising that the atrial concentration of norepinephrine is significantly greater than the ventricular concentration.

Anatomical findings[6–8] and studies on the effect of electrical stimulation of sympathetic cardiac nerves on ventricular contractile force[9,10] suggest, however, that adrenergic nerve endings are distributed

throughout the ventricles as well as the atria, a conclusion which is supported by the high concentrations of norepinephrine found throughout the ventricular muscle.

PARKHURST A. SHORE
VICTOR H. COHN, JUN.
BENJAMIN HIGHMAN
HARRIET M. MALING

National Heart Institute
and
National Institute of Arthritis
and Metabolic Diseases,
National Institutes of Health,
Bethesda, Maryland.

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Polymorphism in the β-Globulins of Sheep

POLYMORPHISM in the β-globulins of cattle, giving five phenotypes[1,2], and in humans giving three phenotypes (Smithies, O., personal communication), has been described. The present communication concerns polymorphism in the β-globulins of the serum of sheep, giving to date eight phenotypes. The phenotypes are indicated diagrammatically in Fig. 1. The conditions for obtaining these serum protein differences by starch gel electrophoresis are similar to those described for cattle sera[1] except that 5–6 hr. is required to achieve adequate resolution of the β-globulins; in this time the trailing edge of the albumin zone migrates 6–7 cm.

It seems probable that the greatest degree of polymorphism in the serum proteins of mammals will be found in the β-globulins, notwithstanding the fact that serum protein polymorphism was first detected by Smithies in the haptoglobins of humans[3]. A rational nomenclature for the β-globulin phenotypes, sufficiently flexible to cover analogous systems in other animals, is therefore necessary. It is suggested that the proteins contributing to each β-globulin polymorphic system be designated by letters in sequence starting at the β-globulin nearest the albumin, that is, the one with greatest mobility; an intensely staining zone could be designated by a capital letter, a less intensely staining zone by a lower case letter, and the absence of the zone by omission of the letter. Such a phenotype nomenclature which makes no implications about the genotypes, is shown for the sheep β-globulin types in Fig. 1.

The distribution of the eight phenotypes in sheep found by examination of 118 abattoir samples (mixed breeds) was: 17 BcdE, 18 BcD, 13 BCd, 5 B, 2 bCdE, 45 bCD, 3 bCd and 15 cD.

The nature of the zones contributing to the one-dimensional starch gel electrophoretic patterns in sheep was determined by two-dimensional agar-starch gel electrophoresis as used for cattle[1] (Fig. 2).

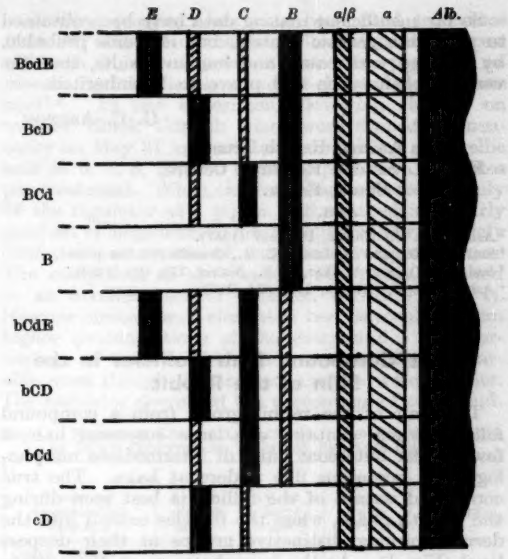

Fig. 1. Diagrammatic representation of β-globulin polymorphism in sheep sera by one-dimensional starch gel electrophoresis. Only a portion of the anodic side of the gel is shown. *B*, *C*, *D* and *E*, β-globulins; *α/β*, zone consisting of β-globulin *A* plus some α-globulin components; *α*, α-globulin components; *alb.*, part of albumin zone. Solid bands, intense staining; cross-hatched, medium or faint staining. For phenotype code, see text

It will be seen that the β-globulins coincide with some of the α-globulin components when the sera are run in starch gel only. Fortunately the α-globulins do not stain sufficiently intensely to obscure the β-globulin pattern, although where β-globulin *B* is absent, α-globulin zones 10 and 11 (Fig. 2) can be seen as background staining. Usually the α-globulin zone 9 (Fig. 2) can be seen as a faint poorly defined zone just behind β-globulin *E*. No 'thread proteins' such as those found in cattle[1] or pigs[4] were observed in sheep.

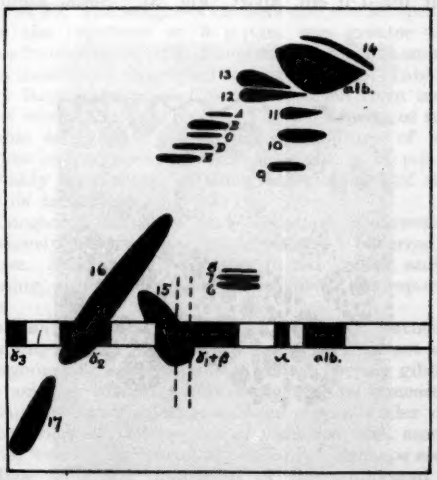

Fig. 2. Diagram prepared after two-dimensional agar/starch gel electrophoresis of type BcdE serum. Broken lines indicate position of original sample insertion in agar. *Alb.*, albumin; *α*, α-globulin (zones 9–13); *β*, β-globulin (zones *A*, *B*, *C*, *D*, *E*); γ_1, γ_2, γ_3, γ-globulins (zones 15, 16 and 17); zones 6, 7, 8, "slow α-globulins"; zone 14, pre-albumin

So far insufficient mating data have been obtained to postulate genetic control, but it seems probable, by analogy with cattle and human results, that the various phenotypes will prove to be inherited.

G. C. Ashton

The Animal Health Trust,
Farm Livestock Research Centre,
Stock, Essex.
Jan. 9.

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Giant Compound Hair Follicles in the Skin of the Rabbit

The hair of the rabbit grows from a compound follicle[1] which contains one large outercoat hair, a few smaller outercoat hairs of intermediate morphology and numerous fine undercoat hairs. The true compound nature of the follicle is best seen during the growth-phase, when the follicles extend into the dermis forming distinctive groups at their deepest level (Fig. 1). As the hair shafts reach the surface the compound follicle is subdivided by fibrous septa into smaller groups which surround the large outercoat hair. Each of these smaller groups of undercoat hairs penetrate the skin through a common follicle opening, whereas the outercoat hair has a separate opening.

If the hair is plucked out in the quiescent phase it can easily be separated into tufts that correspond to the pattern of the compound follicle (Fig. 2). This act of plucking out the hair initiates its regrowth cycle, and in the pigmented rabbit evidence of regeneration is first observed on about the seventh day, when the skin begins to darken as a result of the formation of pigment in developing follicles[2]. Between this period and the time the hair penetrates the skin on about the fourteenth day, the skin surface shows small discrete elevations uniformly distributed about 0·5–1·0 cm. apart (Fig. 3). Serial sections

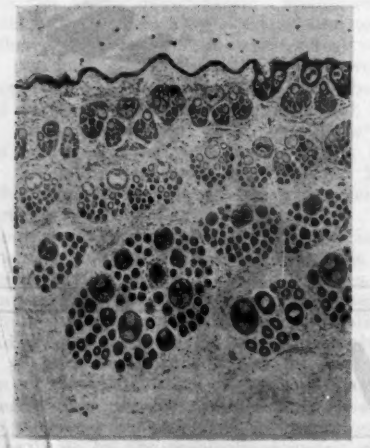

Fig. 1. Section, cut across the slope of the hair, of rabbit skin in the twelfth day of the regrowth cycle showing one giant follicle and several normal compound follicles. The subdivision of the follicle with the single isolated outercoat hair is seen as the hair shafts near the surface. (× c. 30)

Fig. 2. Drawing of a tuft of rabbit hair showing the large black outercoat hair

Fig. 3. Photograph of the skin of the rabbit on the twelfth day of the cycle showing nodular elevations caused by the giant follicles. (× 1½)

through the skin at this time show that these elevations are caused by giant compound follicles (Fig. 1) which contain several outercoat hairs and many more undercoat hairs than the normal follicle. In the chinchilla rabbit the central hair is large, thick and uniformly black in contrast to the banded black-and-white outercoat hair of the normal follicle.

These giant follicles have always been seen in the chinchilla rabbit when the hair is regenerating, and observations indicate that they are present in many other types of rabbit, including the agouti, albino, rex and angora strains.

H. J. Whiteley

Department of Pathology,
Welsh National School of Medicine,
Cardiff.
Jan. 10.

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Capacitation of Mammalian Spermatozoa

It has been previously shown that spermatozoa of the rat and rabbit require to undergo some form of physiological preparation within the female reproductive tract before they are capable of fertilizing eggs[1]. This preparatory process, known as capacitation, has hitherto lacked more precise description.

Recently, we have obtained evidence that the acrosome is removed from the spermatozoon head before the entry of the spermatozoon into the egg. Modification and loss of the acrosome have been observed in living spermatozoa of the golden hamster, guinea pig and Libyan jird recovered in the fluid from the Fallopian tubes and in the cumulus oophorus. Further, spermatozoa found embedded in the zona pellucida of guinea pig and golden hamster eggs, lying free within the perivitelline space of golden hamster and jird eggs, and entering the vitellus of golden hamster and Chinese hamster eggs all lacked their acrosomes. In no instance was a spermatozoon that retained its acrosome found within any part of an egg. These and other observations indicate that, in the normal course of events, removal of the acrosome probably occurs at the surface of the zona pellucida.

Clearly the acrosome is not involved in the penetration of the egg, and its function is probably restricted to assisting the passage of the spermatozoon through the cumulus oophorus. Loss of the acrosome exposes a specialized region of the nuclear membrane, the perforatorium, and we believe that this is the organelle that enables the spermatozoon to make its way through the zona pellucida.

We therefore conclude that acrosome removal is a prerequisite of fertilization, and that the induction of those changes that lead to detachment of the acrosome represents capacitation.

A preliminary report of these investigations was presented at the ninth International Congress for Cell Biology, 1957; a detailed account is being submitted for publication elsewhere.

C. R. Austin
M. W. H. Bishop

National Institute for Medical Research,
Mill Hill,
London, N.W.7.
Dec. 20.

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Effect of Gibberellic Acid on Fruit Set and Berry Enlargement in Seedless Grapes of *Vitis vinifera*

Although an increased set of fruit as a result of gibberellic acid or related compounds has been observed in tomatoes[1], there are insufficient data concerning the effect of this chemical on enlargement of fruit in any plant.

In several experiments performed at Davis, California, in 1957, an increase in fruit set and/or fruit size resulted from treatment with gibberellic acid. The potassium salt of gibberellic acid, supplied by Merck and Co., contained about 80 per cent active ingredient. 'Dreft' was used for a wetting agent. The procedure for determining average weight per berry, percentage of total soluble solids, and percentage of acid has been described previously[2].

Girdling (ringing) has long been used to induce fruit set and enlargement in Black Corinth grapes, although in recent years it has been partially replaced by applications of 4-chlorophenoxyacetic acid[3,4]. In one experiment flowering clusters on mature Black Corinth vines were dipped momentarily on May 31 into solutions containing gibberellic acid at 0, 1, 5, 20, 100 or 500 p.p.m., ten replicates per treatment. When the fruit was collected on July 30 the regulator at 1 p.p.m. had resulted in a fairly good set of large berries, but the clusters were straggly owing to occurrence of small undeveloped berries. The compound in the range 5–500 p.p.m. resulted in an excellent set of enlarged berries (Table 1). Heavier clusters and elongated berries resulted from higher concentrations of the compound. The percentage of total soluble solids was usually comparable even though the treated clusters were heavier. The regulator decreased the percentage of total acid.

Table 1. Effect of Gibberellic Acid on Yield and Composition of Black Corinth and Thompson Seedless Grapes

Treatment, concentration (p.p.m.)	Av. wt./cluster (gm.)	Av. wt./berry (gm.)	Total soluble solids (per cent)	Acid (per cent)
Black Corinth				
0 (not girdled)	91·6	0·26	23·4	1·75
1 (not girdled)	206·5	0·47	23·6	1·50
5 (not girdled)	258·0	0·58	23·9	1·29
20 (not girdled)	366·6	0·78	21·3	1·34
100 (not girdled)	391·3	0·99	23·0	1·26
500 (not girdled)	500·7	1·31	22·6	1·30
Thompson Seedless				
0 (not girdled)	0·66	1·59	22·4	0·79
5 (not girdled)	0·95	1·91	23·2	0·78
20 (not girdled)	1·46	2·71	18·9	0·83
50 (not girdled)	2·15	3·15	17·6	0·94
0 (girdled)	1·10	2·26	23·0	0·74

Girdling has been shown to increase size of berry in Thompson Seedless[3]. On June 11, after berry set was completed (berry shatter stage), mature, ungirdled Thompson Seedless vines were heavily sprayed with solutions containing gibberellic acid at 5, 20 and 50 p.p.m., four vines per treatment. One group of vines was ungirdled and unsprayed, and another girdled but unsprayed. Fruit was harvested on August 26. The weight of clusters and berries sprayed with the regulator at 5 p.p.m. was greater than those from unsprayed and ungirdled vines, but smaller than those from unsprayed but girdled vines (Table 1). Very large clusters and berries resulted from treatment with 20 and 50 p.p.m. The percentage of total soluble solids was lowest and percentage of acid highest in fruit sprayed with compound at 50 p.p.m., probably as a result of the greater weight of fruit in this treatment.

Elongation of stems and hastening of flowering, previously reported for other plants[1,5] occurred in grapes. Accelerated coloration in red grapes, and in ripening, occurred in many experiments not reported here.

It has been suggested that levels of naturally occurring auxins are of importance in fruit set and enlargement[6,7]. Perhaps naturally occurring gibberellic acid or related compounds may be concerned in fruit set and enlargement on grapes under discussion herein. Berry size of varieties with seeded berries was not increased appreciably. Perhaps seeds produce sufficient quantities of the compound to result in almost maximum berry enlargement. The occurrence of 'gibberellin-like' substances in many seeds[8] suggests an investigation be made on the regulator in grape seeds.

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Robert J. Weaver
Department of Viticulture and Enology,
University of California,
Davis, California.
Dec. 18.

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Effects of Kinetin on Cell Division in *Paramecium caudatum*

Kinetin (6-furfurylaminopurine) has been reported to stimulate cell division in excised tobacco pith tissues[1], in onion root tips[2] and, most recently, in sarcoma tumour cells[3]. Lettré and Endo[4] found no changes in cell division or tumour growth with kinetin in cultures of normal or malignant human and animal cells. They concluded that these tissues either did not possess a kinetin-like trigger for cell division, or contained optimal levels of such a substance.

This communication presents some of the findings on changes in rates of division induced by kinetin in a free-living organism, *Paramecium caudatum*.

Stock cultures of *P. caudatum* (obtained from the Hebrew University Department of Zoology) were maintained as clones on hay infusion medium at 25° C. in the dark. Families were begun in depression slides containing a single animal in two drops of hay infusion plus four drops of sterilized tap water. Treatment slides contained hay infusion with kinetin (kindly supplied by Prof. F. Skoog), dissolved in sterilized tap water and added to the hay infusion at the time of treatment. All slides—treatments and controls—had equal proportions of infusion and water and were adjusted to *p*H 6·5–6·8.

The findings reported here were obtained from tests with isolation cultures using the following procedure. A single individual was isolated in a depression slide. After two divisions, the four resulting animals were transferred each to a new slide—one control and three different concentrations of kinetin solution. Thus each experimental observation was matched by its own control, originated from the same individual. The number of divisions per slide was recorded every 24 hr., after which two animals were transferred from each slide to new medium.

Table 1. Comparisons of Division-Rates of *Paramecium caudatum* in Control Medium and in Various Concentrations of Kinetin

Concentration of kinetin (mgm./l.)	No. of 24-hr. period comparisons	Mean No. of divisions/24 hr.		Proportion $\bar{d}/\bar{c}$	$t\bar{c} - \bar{e}$
		Control $\bar{c}$	Kinetin $\bar{e}$		
0·25	60	1·32	1·48	1·12	−1·57
0·5	18	1·24	1·72	1·39	−2·71*
1·0	24	1·42	2·06	1·45	−3·20†
1·5	18	1·24	2·20	1·77	−3·88†
2·0	24	1·42	1·99	1·40	−2·29*
2·5	18	1·24	1·91	1·53	−2·41*
3·0	24	1·42	1·86	1·31	−2·35*
4·0	21	2·36	2·50	1·06	−0·42
10·0	53	1·60	1·25	0·78	3·34†
17·0	35	2·14	0·94	0·44	6·53†
33·0	30	2·03	0·89	0·44	6·80†

* Significant at 0·05 level. † Significant at 0·01 level.

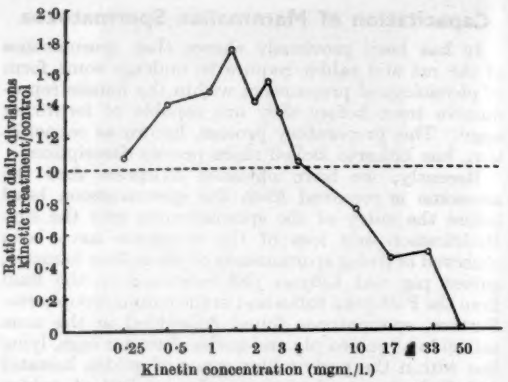

Fig. 1. Variations in division-rate of *P. caudatum* with changing kinetin concentration

Readings were made for three days in succession: at the end of these the experiment was discontinued and a new set begun. Mean division-rates of treatments and controls were compared and their differences tested for significance by Student's *t* test for the mean of differences.

In order to test a possible effect of the kinetin on the bacterial populations of the culture media during the 24-hr. periods, hay infusion samples—with and without kinetin—were tested on agar and in bouillon. No gross changes were observed in either size or constitution of the bacterial populations.

Table 1 shows appreciable changes in mean daily division frequencies (for 3-day periods) in cultures containing a variety of concentrations of kinetin. 100 mgm./l. kinetin caused death of all *Paramecia* within ½ hr., while 50 mgm./l. blocked division completely without being lethal. (The animal remained alive during the 3-day period but did not divide.) Between 33 mgm./l. and 0·25 mgm./l. we found a sensitive range of responses from significant elevations to severe inhibition of cell multiplication (Fig. 1). As the dose of kinetin decreased from 33 to 4 mgm./l., the ratio of experimental to control divisions rose from 0·44 to approximately 1. The ratio rose steadily above 1 as the concentration dropped from 4 to 1·5 mgm./l., and fell steadily back to 1 as the concentration dropped from 1·5 to 0·25. It is interesting to note that the concentrations found to be most active in our material, as well as in onion and tumour cells[2,3], have all been of the same order of magnitude.

Tests are under way to determine the time of onset of the effect of kinetin during the life-cycle of the protozoan cell.

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Ruth Guttman
Avigdor Back
Department of Experimental Medicine
and Cancer Research,
Hebrew University–Hadassah Medical School,
Jerusalem, Israel.
Dec. 23.

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Chromosomes and Chromocentres in the Nuclei of *Halidrys siliquosa* (L.) Lyngb.

DIVIDING nuclei in the apical groove of *Halidrys siliquosa* show large numbers of small chromosomes, and at metaphase more than 55 have been counted (Fig. 1*c*, *d*, *e*). Metaphase plates at the first three divisions in developing antheridia have given counts of the order of 30 chromosomes (Fig. 1*b*), and one at the first division of the oogonium of approximately 28 chromosomes. This indicates that the thallus of *H. siliquosa* is diploid with meiosis at the first division of the gametangia, as is usual in the Fucales[1].

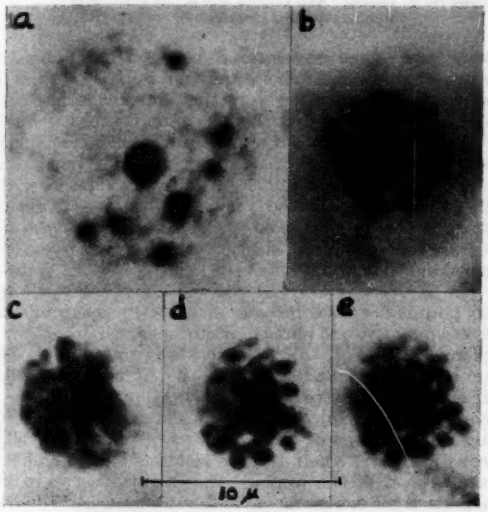

Fig. 1. Photomicrographs of nuclei of *Halidrys siliquosa*, all to the same scale. *a*, Acetocarmine smear of a nucleus of a medullary cell showing the nucleolus and seven chromocentres; *b*, Feulgen-stained metaphase plate at the first division of the antheridial initial: this plate gave a count of 34; *c*, *d*, *e*, three optical planes of a metaphase plate of a dividing meristoderm cell in the apical groove. This plate gave a count of 55 chromosomes plus several more the number of which could not be accurately determined, lying in a higher plane. The illustration was prepared by W. H. Smithson from untouched negatives

The resting nuclei of *H. siliquosa* contain several Feulgen-positive granules about 1μ in diameter (Fig. 1*a*) and slightly larger than the chromosomes, as has been described by le Touzé[2] and Roy[3]. The number of granules varies from cell to cell but is usually between 2 and 9, whether the cell is diploid, as in the somatic cells, or haploid, as in the developing gametangia. Such variability in number and independence of chromosome complement is a characteristic of chromocentres[4]. It seems possible that the eight chromosomes recorded by Moss and Elliot[1] in both somatic and reproductive cells of *H. siliquosa* correspond to these bodies, which are probably chromocentres.

A more detailed account of this work will appear elsewhere[5].

MARGARET NAYLOR

Department of Botany,
The University,
Hull.
Dec. 17.

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Cytology of Indian Species of *Artemesia*

CONSIDERABLE cytological work has been done on the genus *Artemesia* in North America[1,2], Japan[3] and Europe (cf. Darlington and Wylie[4]); but so far nothing has been known about the Indian species. The genus, besides being of medicinal importance, is extremely interesting from the cytotaxonomic and cytogeographic points of view. It was therefore thought worth while to work out the available species of the genus. Following the conventional taxonomic treatment we have worked out 11 species out of 27 reported by Hooker[5] from the Indian subcontinent. In the present investigation the various species have been primarily sampled from the north-western Himalayas; but material from the eastern Himalayas and other areas has also been studied. This has been supplemented with the study of herbarium specimens. Most of the species have been worked out for the first time. In addition to the 11 Indian species, the chromosome number in *A. vulgaris* Linn. from Germany has also been investigated. The chromosome numbers are given in Table 1.

Table 1

Species	Chromosome No. *n*	Chromosome No. *2n*
A. scoparia Waldst. and Kit.	8	16
A. absinthium Linn.	9	—
A. glauca Pall.	18	36
A. laciniata Willd.	—	18
A. maritima Linn. (Kashmir)	18	—
A. maritima Linn. (Japan)	27	—
A. parviflora Roxb.	9	18
A. roxburghiana Bess.	9	18
A. roxburghiana Bess.	18	36
A. tournefortiana Reichb.	9	18
A. vestita Wall.	9	18
A. vulgaris Linn. (Germany)	—	16
A. vulgaris Linn. (India)	18	—
A. vulgaris Linn. (India)	27	54
A. species	9	—

One of the interesting aspects of this study is the cytological situation in Indian *A. vulgaris*. In this connexion, it is pertinent to mention that *A. vulgaris* Linn. was originally described from northern Europe[6]. The chromosome number of this species ($2n = 18$) was worked out by Weinedel as early as 1928 (cf. Darlington and Wylie[4]). In view of all the subsequent work it is reasonable to revise the above number to $2n = 16$[1,7–10]. This is fully borne out by the present investigation since we also found 16 chromosomes in root-tips from seeds collected at Kitzeberg near Kiel (Germany) and sent to us by Prof. H. D. Wulff. This is a significant point, since Indian *A. vulgaris* has been compounded from the basic number 9 and not 8 as expected on the basis of true *A. vulgaris* from Europe. Evidently the former taxon has to be appropriately evaluated taxonomically.

The Indian *A. vulgaris* complex is also interesting phytogeographically. We have found only diploids in the innermost Himalayas, in regions which have undergone a period of glaciation, and often at such places glaciers are still in existence. In the middle Himalayas diploids are at the tops of mountains and perhaps extend farther down. The tetraploid forms occur only at lower altitudes and extend also into the valleys below. In the outer Himalayas the diploids of this complex are either rare or have not been found so far, whereas tetraploid forms are preponderant. The hexaploid forms appear at lower altitudes and have also been found in valleys between the outer Himalayan foothills and the Siwalik Ranges. Furthermore, hexaploid has also moved down to the Nilgiri Hills

(south India) in all probability through Mount Abu. From a study of the herbarium specimens we have reason to believe that hexaploid *A. vulgaris* also occurs in Ceylon, and on the eastern side it has also moved down to the Martaban Mountains in Burma. This is an interesting situation and will be further elaborated later.

A. maritima Linn. is a widespread species. It is distributed from the coasts of Europe and extends throughout saline tracts in north Asia[5]. The species has been reported as diploid from Europe[11] and as hexaploid from Japan[3]. We have confirmed the later report from plants at Yarikhah Drug Farm (Tangmarg, Kashmir) which were raised from seeds imported from Japan. We have, however, found the indigenous plants of the species at Gurez (Kashmir) to be tetraploid.

Fuller discussion will be published elsewhere.

We are grateful to Col. R. N. Chopra and Dr. I. Chopra (D.R.L., Jammu) and Prof. P. N. Mehra (Panjab University) for their encouragement and for affording us facilities to carry on this investigation. Furthermore, we are indebted to Prof. H. D. Wulff (Saarbrucken, Germany) for sending seeds of European *A. vulgaris*.

T. N. Khoshoo

Botany Department,
Panjab University,
Amritsar.

S. N. Sobti

Drug Research Laboratory,
Jammu Tawi.

Jan. 2.

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Chromosome Size in *Lolium temulentum* and *L. perenne*

Morphologically the grasses *Lolium perenne* and *L. temulentum* are markedly dissimilar and easily distinguished from one another. They differ also in habit and in their breeding system; the former is perennial and outbreeding, while the latter is an annual and inbreeding. Despite their divergencies in form and habit, the species can, however, be crossed, and hybrid plants are readily grown from seed produced when *L. perenne* is used as female parent. This was demonstrated by Jenkin[1], who has described the hybrids in detail.

A cytological investigation by Thomas[2] has revealed considerable genetic variation in the size of chromosomes. When the chromosomes were compared in order of size, those of *L. temulentum* were found to be approximately a third longer than those of *L. perenne*. In seeking to explain this variation there are two possibilities to consider. Either there are differences in the amount of genetic material in the chromosomes of the two species or there are genotypic differences between them which affect chromosome size—such as reported between a number of species[3], and even between individuals within a species[4]. Comparisons were therefore made between the parent species and their hybrids to decide which of these possible causes applies in this instance. This was done by comparing the lengths of particular chromosomes in the nuclei of the parent species with their lengths in hybrid nuclei. The measurements were made in sectioned preparations of root tips which had not been subjected to pre-treatment of any kind that would affect the length of chromosomes.

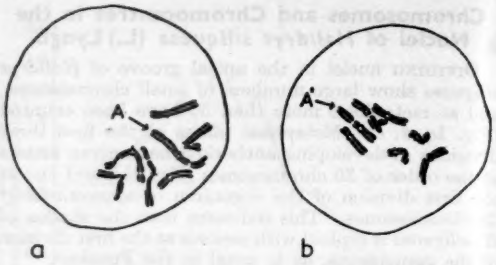

Fig. 1. The first pollen grain mitosis in (*a*) *Lolium temulentum* and (*b*) *L. perenne*. The average chromosome length is about a third greater in *L. temulentum*. (× 1,000)

In *L. temulentum* the chromosomes are too long to permit accurate measurement of complete nuclei. Consequently, one particular chromosome in the complement, *A* (Fig. 1*a*), was measured in suitable cells in the parent plants and in the hybrids. Similar measurements were made of the *L. perenne* chromosome, *A′* (Fig. 1*b*). The chromosomes were identified by the positions of their centromeres and secondary constrictions. Their lengths relative to one another are typical of the average length ratio between the chromosome complements of the two species. Whether they are homologous is not certain, but, from their appearance, probable. Sample measurements from within species were homogeneous, so also were those within hybrids. The results, therefore, were pooled and are represented in Fig. 2.

The average length of each chromosome within cells of the parent species is not significantly different from that in the cells of the hybrid, and the ratio *A*/*A′* calculated from measurements in the parent cells (1·3) is much the same as the ratio calculated from hybrid cells (1·4). There is, therefore, no evidence that the size difference is to any extent genotypically determined. The difference, we conclude, is at least mainly due to additional genetic material in the chromosomes of the inbreeding species.

This interpretation is corroborated by our own and Thomas's observations of meiosis in the pollen mother

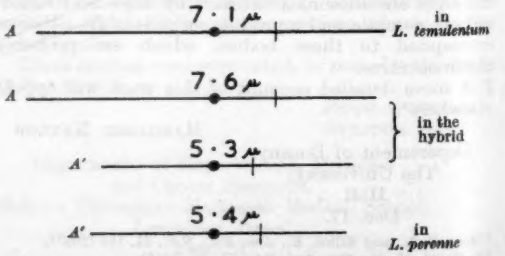

Fig. 2. The average lengths of a chromosome from each species in the nuclei of the parent cells and in those of the hybrid cells. The lengths are based on ten measurements in each case. Lengths of the secondary constrictions are not given

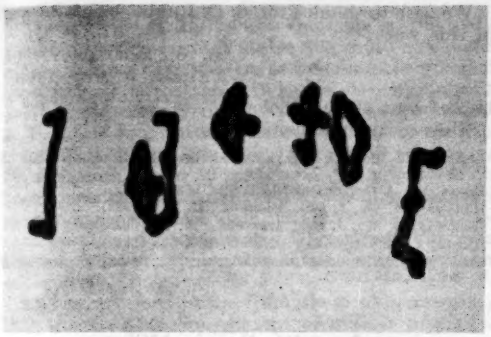

Fig. 3. First meiotic metaphase in a pollen mother cell of the *Lolium* hybrid showing asymmetrical bivalents. (× 1,750)

cells of the hybrids. At pachytene unpaired loops indicating deficiency or excess of genetic material are frequent. At metaphase the persisting inequality of chromosome size is manifested by the distinct asymmetry of up to six of the bivalents (Fig. 3). These asymmetrical bivalents are, with respect to long and short arms, orientated at random. There is no evidence for 'affinity'.

There may well be a causal relation between the variation in the amount of genetic material in the two species and the difference in their breeding systems. We have no evidence to show whether this is so, or how it may operate.

We wish to thank Mr. Beddows, of the Welsh Plant Breeding Station, Aberystwyth, for seeds of *L. temulentum*.

BERYL NAYLOR
H. REES

Agricultural Research Council
Unit of Biometrical Genetics,
Department of Genetics,
University of Birmingham.
Jan. 3.

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Fortifying Influences of Coconut Milk and Cow's Urine on 2,4-Dichlorophenoxyacetic Acid in the Control of Button Shedding in the Coconut

DURING several trials carried out by us with different hormones in the control of button shedding in the coconut, there was clear indication that, while sprays of 2,4-dichlorophenoxyacetic acid in 30 p.p.m. strength helped in securing a heavy set of buttons, quite a large number of them turned out to be abnormal or barren and markedly reduced in size when they attained maturity. This was observed to be particularly pronounced when pre-fertilization sprays were given. Although a change from pre-fertilization to post-fertilization sprays brought about a considerable reduction in the occurrence of abnormal and barren nuts and an appreciable improvement in the size of nuts at maturity, it did not help in completely offsetting the defects. It was therefore thought worth while to test the utility of coconut milk and cow's urine in overcoming these defects, since these might, by virtue of their growth-promoting properties, be expected to fortify the action of 2,4-dichlorophenoxyacetic acid, acting synergistically[1] or in some other way.

Preliminary trials were accordingly undertaken by using mixtures of coconut milk or cow's urine (which by themselves were inactive in improving button setting) with equal volumes of 2,4-dichlorophenoxyacetic acid in order to ascertain to what extent these would serve as an improvement. The results of these trials have indicated the interesting beneficial effect in using the mixtures, as these have not only helped to increase the percentage set of nuts and their overall development, but also reduced the incidence of barren nuts compared with the results secured with sprays of 2,4-dichlorophenoxyacetic acid alone, as may be seen from the results presented in Table 1.

Table 1

Characters determined	Control (water spray)	Sprays of 2,4-D alone	Sprays of 2,4-D mixed with coconut milk	Sprays of 2,4-D mixed with cow's urine
Mean percentage set of good nuts	7·62	26·20	44·83	31·23
Mean No. of under developed nuts	0·00	9·66	4·33	4·66
Mean vol. per nut (ml.)	2,158·33	1,516·67	2,373·33	2,360·83
Mean wt. per nut (gm.)	903·33	619·00	745·40	731·00
Mean wt. of copra per nut (gm.)	171·67	135·00	152·50	150·33

2,4-D = 2,4-dichlorophenoxyacetic acid

It is quite clear that there is a significant fortifying influence when 2,4-dichlorophenoxyacetic acid was used with a supplement of coconut milk or cow's urine and that the former proved to be superior in its synergistic effect than the latter.

Further trials are in progress to evaluate the beneficial effects of these and similar other mixtures on a statistical basis. We thank Dr. K. P. V. Menon for his interest in the work and Mr. T. P. Gopalakrishnan for his technical assistance.

S. R. GANGOLLY
K. M. PANDALAI

Central Coconut Research Station,
Kasaragod, South India.
Dec. 5.

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An Infectious Fertility Factor for *Pseudomonas aeruginosa*

ONE unusual feature of the system of sexual recombination occurring in *Escherichia coli* is the infectious nature of the *F* factor. *F* − strains may be readily converted to *F* + by growing the two types together[1,2].

A similar type of sexual recombination to that occurring in *E. coli* occurs in *Pseudomonas aeruginosa*[3,4]. Two of the *Pseudomonas* strains used, 1 and *L*, are interfertile. Furthermore, 1 is self-sterile but *L* is self-fertile with a lower frequency of recombination than the 1 × *L* combination. This situation is comparable to the fertility system occurring in *E. coli K*12. Strain 1 is the equivalent of the *F* − strain, strain *L* the equivalent of the *F* +. Previous attempts to find an infectious fertility factor in *Pseudomonas*

have, however, been unsuccessful. In those experiments, strain 1 and strain *L* auxotrophs were grown together under various conditions and the strain 1 auxotroph afterwards re-isolated. However, in no case were the mating characteristics of strain 1 altered in any way.

It has been previously shown that from crosses of multiple auxotrophic strains of 1 and *L*, auxotrophic recombinants may be isolated which show segregation for the ability to cross with either strain 1 or *L*[3]. Most of the recombinants resemble strain 1 in many of their characteristics, for example, pigment formation and susceptibility to phages. Thus it was thought possible that those recombinants of *L*-like mating ability may show a greater ability to transfer a fertility factor, should it exist.

A doubly auxotrophic recombinant *R*629 ($IV_1^- T_1^- S^s$), *iso*leucine plus valine requiring, tryptophan requiring, streptomycin sensitive and *L* mating type, was obtained from the cross $1(M_1^- T_1^-)$, methionine requiring, tryptophan requiring, $\times L(Ad^- IV_1^-)$, adenine requiring, *iso*leucine plus valine requiring. This recombinant was mixed with a strain 1 auxotroph, $1(T_1^- S^r)$, tryptophan requiring and resistant to streptomycin, in 'Difco' nutrient broth plus 0·5 per cent 'Difco' yeast extract in a ratio of 20 : 1 in favour of *R*629 and grown overnight on a shaker at 37° C. The mixture was plated out on 'Difco' nutrient agar plus 250 μgm./ml. streptomycin to re-isolate the strain 1 auxotroph. Twenty-five colonies of this strain were then picked off and tested to see if overnight growth in the presence of the *L* mating type *R*629 had caused any change in the mating type of $1(T_1^- S^r)$, that is, the ability to cross with either strain 1 or strain *L*. It was found that 20 of the 25 colonies picked had permanently acquired the ability to cross to strain 1, thus resembling strain *L* in their mating ability. This change is analogous to the infection of *F*− strains in *E. coli* *K*12 to become *F*+. The conclusion is drawn that a factor similar to the *F* factor in *E. coli* *K*12 controls fertility in the 1 and *L* strains. This factor has been named *FP*. Thus, strain 1 is *FP*− and strains *L* and *R*629 are both *FP*+.

No reason can be given at present for the failure of strain *L* to infect strain 1 with *FP*. The infectivity of *FP* within strain 1 is irregular. Infection of a $1(FP-)$ strain from a $1(FP+)$ strain under the above conditions occurs only at low frequency and it is possible that the optimum conditions of transfer have not been obtained. The change from *FP*− to *FP*+ has been achieved in a number of different strain 1 auxotrophs. Different lines derived from strain 1 vary in the facility with which they are converted from *FP*− to *FP*+, and one of these lines has proved refractory to this change. It is thought possible that the transfer of *FP* may be under the control of certain hereditary factors. These factors may be also responsible for the segregation of *FP* in recombinant progeny previously observed.

In view of recent experiments[5] on the effect of acriflavine on the conversion of *F*+ to *F*− in *E. coli* *K*12, the action of this substance on *FP* was investigated. No conversion of *FP*+ to *FP*− could be accomplished either by growing the *FP*+ strains in 'Difco' nutrient broth plus 200 μgm./ml. acriflavine for up to 48 hr., or by making *FP*+ strains resistant to 1 mgm./ml. acriflavine.

Attempts were also made to select *FP*− mutants from *FP*+ strains by selection for mutants with higher motility[6]. Repeated trials failed to produce any *FP*− mutants by this method.

We wish to thank Prof. S. D. Rubbo for his interest in this work.

B. W. Holloway
Pamela A. Jennings

Department of Bacteriology,
University of Melbourne.

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Acetylcholine in Milk

Acetylcholine has recently been reported to be present in low concentrations (0·1–1 μgm./ml.) in the milk of various species[1–4]. On the other hand, the mammary gland does not appear to possess a cholinergic innervation[5,6]. Accordingly, the question of the presence of acetylcholine in milk has been re-examined.

Fresh cows' milk, assayed on the frog rectus abdominis muscle preparation, gave varying responses equivalent to 0·1–0·4 μgm. acetylcholine chloride/ml. The activity differed from that of acetylcholine in the following ways: (1) the dose-response curve was much steeper; (2) heating the milk at *p*H 12 and 100° C. for 10 min. (conditions which destroy acetylcholine) caused little or no reduction of the activity; (3) amounts of milk which should have evoked, on the basis of their acetylcholine equivalence on the rectus, a good response from the anti-histamine-treated guinea pig ileum, failed to do so; (4) samples of milk, ashed and dissolved in a volume of water equal to that of the original sample, showed the same activity as the original milk. This suggested that the rectus activity was due to an inorganic constituent of the milk, probably potassium. It was found possible to duplicate the response to the milk with a potassium chloride solution of the same potassium content.

Since acetylcholine might be present in milk in a bound or pharmacologically unavailable form, or in such low concentration as to escape detection unless concentrated, the milk was treated with trichloracetic acid and the basic constituents of the trichloracetic acid supernatant precipitated as reineckates, procedures which release and concentrate acetylcholine from tissues (see ref. 7 for references). The trichloracetic acid was added to a final concentration of 10 per cent (w/v); the precipitate was removed by centrifugation, extracted with 10 per cent (w/v) aqueous trichloracetic acid and again removed by centrifuging. The combined supernatants (trichloracetic acid extract) were extracted with ether to *p*H 4·5 to remove the trichloracetic acid and the ether removed in a stream of air before testing. Bases were precipitated as reineckates by the addition of excess saturated ammonium reineckate and converted to the chlorides by passing the reineckates in 50 per cent (v/v) aqueous acetone through a column of the anion exchanger 'Deacidite *FF*' (Permutit Co., Ltd., London, −16 +50 mesh) in the chloride form. Control experiments with acetylcholine gave an overall recovery of about 50 per cent for this procedure. Potassium and choline were estimated in the various fractions by flame photometry and by the enneaiodide method[8] respectively.

The activity of the various fractions on the rectus was, in every case, alkali-stable and could be equated with a solution of potassium and choline of equivalent strength. Whereas the activity of the original milk was almost entirely due to potassium, the activity of the base concentrate was mainly due to choline; that of other fractions was due to both substances. On the ileum, about 80 per cent of the activity of the base concentrate was abolished by the anti-histaminic pyrilamine maleate (1 μgm./ml.), and was attributed to histamine; the remaining 20 per cent was more than accounted for by the choline content of the concentrate.

The conclusion drawn from these experiments is that the acetylcholine content of milk is negligible, and that the contractions of the frog rectus and guinea pig ileum preparations produced by milk or milk extracts are due to the potassium, choline and histamine content of milk.

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V. P. Whittaker

Agricultural Research Council
Institute of Animal Physiology
Babraham, Cambridge.

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Use of Tidal Streams by Migrating Elvers (*Anguilla vulgaris* Turt.)

Each spring large numbers of elvers enter the Dutch Wadden Sea through the Marsdiep tidal inlet and migrate to the places where freshwater is discharged, such as at the sluices in the enclosing dam of the Ijssel Lake at Kornwerderzand and Den Oever (Fig. 1).

To study the migration in this tidal area, catches were made in the Texelstroom and the Marsdiep with a 1 m.² ring-trawl during the years 1955, 1956 and 1957, using a small cutter. The catches consisted of horizontal surface hauls of ¾ hr. each, mostly made at night, during the flood as well as during the ebb. During both tides they were carried out alternately in the direction of the North Sea and in the direction of the inland waters, the speed of the boat being approximately constant in relation to the flowing water.

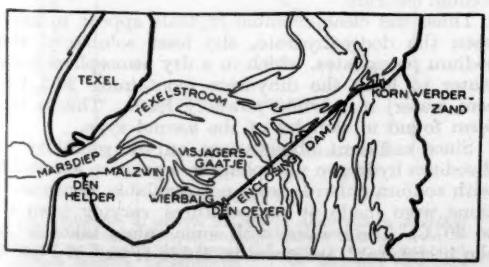

Fig. 1

If the elvers, which rise to the surface at night, do indeed swim actively from the North Sea to the freshwater, we would expect a greater yield of the hauls made towards the North Sea than of those made in the opposite direction. Actually, however, no significant differences in yield between hauls in either direction were found. This seems to exclude the possibility of an active migration of elvers swimming towards the freshwater.

A very significant difference was found, however, between the catches made during the flood tide and the ebb tide, respectively. Three to ten times more elvers were caught during the flood. This means that the greater part of the animals leave the surface during the ebb.

Therefore, during the spring of 1957, catches were also made at a depth of 8 metres, at places where the total depth was 10–20 metres. Also at this depth the yields were much larger during the flood than during the ebb. Hence, we must suppose that during the ebb tide the elvers are to be found near or on the bottom.

It follows from these observations that the elvers most probably use the tidal streams for their migration; they are transported in the direction of the inland waters by the flood, and go to the bottom during the ebb in order not to be carried back to sea.

By fishing in the day-time we obtained evidence that in daylight also this method of migration is used; however, the elvers do not reach the surface then. We have not yet established whether during the ebb the elvers dig themselves into the sand or swim against the current close to the bottom.

The migration principle described above is in accordance with a hypothesis developed theoretically by Van Heusden[1], who also assumed a phase of pelagic transport of the elvers by the flood tide and a phase of active swimming close to the bottom against the ebb tide.

On the other hand, Deelder[2] found no conclusive differences between ebb and flood catches from a ship anchored fairly near the sluices (Wierbalg, Visjagersgaatje) in the years 1947–50. The discrepancy between his and our results, however, can probably be explained as follows.

From our observations in 1957 it was established that the great majority of the elvers passed the Marsdiep during the end of February and the beginning of March. From the observations kindly put at our disposal by the Dienst van de Zuiderzeewerken, we learned that the elvers arrived a full month later at the sluices of Den Oever. Hence, there must be an area between the Marsdiep and Den Oever where the migration suffered a temporary delay; in this area an important accumulation of elvers should take place. Catches made in the Wierbalg during the period in question indeed gave yields which exceeded those in the Marsdiep and the Texelstroom by approximately a hundred times. Moreover, the yields of the ebb and flood catches did not differ significantly. We assume, therefore, that Deelder's results refer to catches made in an area where the migration has temporarily come to a standstill.

The assumption of a temporary stagnation of elver migration in front of the freshwater outlets is also in accordance with the observation of Deelder[3] that at the sluices of Den Oever the elvers at the beginning of the season do not show any preference for freshwater, whereas after some time they are strongly attracted by it.

As regards the use of tidal streams by the elvers migrating from the open sea to the area of stagnation, an important problem arises. How do they discriminate between the flood and the ebb?

Van Heusden[1] supposed that differences in salinity play an important part. One could imagine, for example, that the elvers remain on or near the bottom so long as the stream (mostly the ebb[4]) carries water of lower salinity, whereas they leave the bottom as soon as the stream (mostly the flood[4]) provides water with a higher salinity. Our observations have not progressed sufficiently to allow a conclusion in this respect; probably also the water temperature exerts a certain influence.

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F. Creutzberg

Zoological Station,
Den Helder,
The Netherlands.
Nov. 12.

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Relationship between Aggressiveness and Egg Production in the Domestic Hen

A relationship between position in the peck order and egg production in the hen was first reported by Sanctuary[1] (quoted by Guhl[2]), who found that the birds in the upper half of the peck order laid more eggs than those composing the lower social level. This observation was confirmed by Guhl[2], who obtained a correlation of 0·26 (P, 0·01) between the number of birds pecked in the peck order and the number of eggs laid by 96 White Rock pullets.

The following observations were made on an Australorp flock maintained on the Animal Husbandry Farm of the University of Queensland. The experiment involved the measurement of aggressiveness of pullets in two groups, the first in battery laying cages and the second in an intensive shed on deep litter.

All birds were scored against panels of 10 birds under standard conditions[3]; 6 such panels were used.

There seemed to be no relationship between aggressiveness score and egg production for birds housed in laying cages and, while there appeared to be some effect of aggressiveness on egg productivity in intensively housed birds, it did not appear to be linear. It seems that the majority of birds with low egg production also have low aggressiveness scores. This can similarly be discerned by inspection of Guhl's[2] scatter diagram.

Since Collias[4] has reported that position in the peck order is one of the factors which makes for success in single encounters of the type used in scoring aggressiveness, and correlations between scores and peck order position in assembled groups of these birds have been demonstrated, it would appear that the low aggressiveness scores are reflecting low peck order position in the intensively housed birds. Low position in the peck order is related to low priority at the feed troughs, nests and roosting places[5], and it would appear that it is the rather harsh environmental conditions to which these birds are subjected that is responsible for their low egg production and thus for the observed correlation.

Blyth[6] considered the shape of the egg production frequency distribution to be fundamentally normal although in fact it is skewed with a long 'tail' towards zero. She considered this to be the result of factors not specifically concerned with egg production. The frequency distribution for egg production of sisters housed under three environments, caged, semi-intensive and intensively on deep litter, shows an increasing degree of negative skew from cage to intensive. This suggests that position in the peck order is certainly one of these factors, though the fact that the 'tail' is still present in the caged birds suggests that it is not the only factor. Recent work[7] involving the feeding of antibiotic and vitamin B_{12} supplements has been effective in raising the egg production of low-producing birds while apparently leaving the high-producing birds unaffected. This suggests that this treatment has been effective in removing even more of the 'tail' from the distribution.

G. McBride*

Dept. of Animal Husbandry,
Veterinary School,
University of Queensland.

* Present address: Institute of Animal Genetics, University of Edinburgh, West Mains Road, Edinburgh.

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Sodium Phosphate Crystals on Salt Fish

Several times in the past few years, samples of undried or partly dried salt cod with an accumulation of crystals appearing over the surface have been submitted to this laboratory for examination. These deposits did not resemble either the sodium chloride or the calcium sulphate crystals often found on salt fish. One sample had many water-clear rhombic crystals with edges 2 cm. or more long. Others had a second type of crystals: they were dry, powdery white, and thickly scattered over the surface. Curiously, chemical analysis indicated that both types of crystals contained large proportions of phosphate and sodium, a small amount of chloride, but no sulphate, calcium or magnesium.

When the clear rhombic crystals were exposed to the dry laboratory atmosphere for a day, they changed into a white powder, similar to the second type. This powder contained 20·0 per cent moisture and appeared to be the dihydrate of disodium hydrogen phosphate. On analysis of the sample dried at 105° C., the sodium, phosphate and chloride contents indicated 96·0 per cent disodium hydrogen phosphate with 3·0 per cent sodium chloride.

Thus, the clear rhombic crystals appear to have been the dodecahydrate, the least soluble of the sodium phosphates, which in a dry atmosphere loses water to form the dihydrate (containing 20·2 per cent water) in a white powdery form. This is the form found in samples of the second type.

Since sufficient observations on the solubility of disodium hydrogen phosphate in a solution saturated with sodium chloride were not available, determinations were made at temperatures varying from 0° to 20° C. The results, with some values taken from the literature[1,2], are given in Table 1, and in Figs. 1 and 2.

Table 1

Temperature (° C.)	System $Na_2HPO_4.12H_2O$–NaCl–H_2O Solubilities (gm./100 gm. H_2O)						System $Na_2HPO_4.12H_2O$–H_2O Solubilities (gm./100 gm. H_2O)		
	Na_2HPO_4			NaCl			Na_2HPO_4		
	A	*B*	Average	*A*	*B*	Average	*A*	*B*	Average
0·3	0·579	0·586	0·583	35·41	35·22	35·31	1·76	1·78	1·75
2·5	0·669	0·658	0·664	35·68	35·67	35·68	1·96	1·97	1·97
4·8	0·899	0·900	0·900	35·67	35·60	35·64	2·28	2·28	2·28
10·1	1·77	1·77	1·77	35·40	35·36	35·38	3·60	3·26	3·43
14·5	3·09	3·11	3·10	34·79	34·87	34·83	4·82	4·82	4·82
21·6	7·11	7·08	7·10	33·44	33·41	33·43	8·14	8·15	8·15

Values *A*, equilibrium approached from higher temperatures; values *B*, equilibrium approached from lower temperatures

Fishery salt contains only traces of phosphate. On the other hand, and in agreement with other results[3,4], fresh cod muscle was found to contain 0·2 per cent total phosphorus, of which 0·16–0·17 per cent is inorganic phosphate. Assuming a moisture content of 81 per cent, this would correspond with a concentration of 0·97–1·13 gm. anhydrous disodium phosphate per 100 gm. water. In undried salt fish this figure should remain unchanged: only dry salt is added during salting, and the concentration of phosphate in the water held by the fish should be the same as in the pickle lost, unless some phosphate is held by the tissue. Such a solution, saturated with sodium chloride, would also be saturated with the disodium phosphate dodecahydrate at about 4° C.

After partial drying, the temperature of saturation with respect to the phosphate would be higher. In one sample of slightly dried heavy-salt fish, analysis for moisture and phosphate indicated that this temperature would be about 11° C. Drying therefore favours the formation of the crystals.

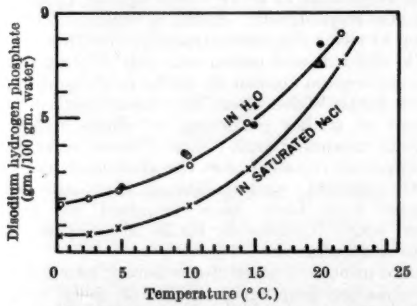

Fig. 1. Solubility of disodium hydrogen phosphate dodecahydrate in water and in saturated sodium chloride solution from 0° to 20° C. ●, in water, data from I.C.T. (ref. 1); ○, in water, experimental; ×, in saturated sodium chloride solution, experimental; △, in saturated sodium chloride solution (ref. 2)

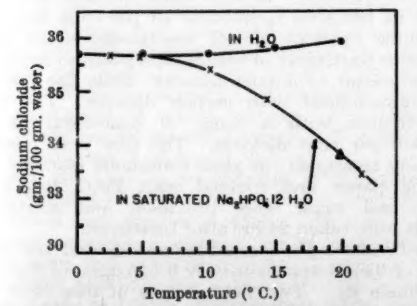

Fig. 2. Solubility of sodium chloride in water and in saturated disodium hydrogen phosphate dodecahydrate solution from 0° to 20° C. ●, in water, data from I.C.T. (ref. 1); ×, in saturated phosphate solution, experimental; △, in saturated phosphate solution (ref. 2)

A slightly alkaline *p*H (about 7·5–8) is also required for the formation of disodium hydrogen phosphate crystals. While the fish itself is almost neutral, most solar fishery salts are alkaline, and any slight spoilage (which often occurs with medium-salt fish) would also tend to increase the *p*H.

Thus the occurrence of these crystals on partially dried salt bulk fish, particularly while in cool storage or during transportation in the winter months, is readily explained. Its higher incidence with medium-cure fish probably depends on the susceptibility to rapid spoilage and the consequent elevation of the *p*H in this type of salt fish.

The effect of the sodium phosphate on the solubility of sodium chloride should also be noted. At 20° C. the solubility of the latter is only 93·9 per cent of the value in the absence of the phosphate. This explains why, in practice, salt fish is not often found in which the water is completely saturated with respect to sodium chloride when the solubility in salt-water systems only is considered, as is usually done.

W. J. Dyer
Doris I. Fraser
J. R. Dingle

Fisheries Research Board of Canada,
Technological Station,
Halifax, Canada.
Nov. 8.

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Effect of Phenobarbitone on the Requirement of *Lactobacillus leichmannii* for Vitamin B_{12}

When sodium phenobarbitone (2 mgm. per ml.) is added to the medium used in a modified method for the microbiological assay of vitamin B_{12} with *Lactobacillus leichmannii* as test organism, an increase occurs in the amount of growth produced by small amounts of vitamin B_{12}[1]. This communication describes results which suggest that phenobarbitone has a specific vitamin B_{12}-sparing action under the experimental conditions previously described[1]. The strain of *Lactobacillus leichmannii* used was supplied by the National Collection of Industrial Bacteria, and is derived from the American Type Culture Collection strain No. 4797.

Table 1. The Effect of Phenobarbitone on the Growth of *Lactobacillus leichmannii* in the Presence of Vitamin B_{12}

Exp. No.	Incubation time (hr.)	Mgm. sodium phenobarbitone per ml. medium	mμgm. vitamin B_{12} added per 5 ml. medium: 0·025	0·05	0·1	0·2	0·4	0·5	0·6
			mμgm. vitamin B_{12} required to produce the same amount of growth in the absence of phenobarbitone						
1	24	2·0	0·12	0·24	0·44	0·64	1·1	—	1·6
2	24	2·0	0·06	0·12	0·22	0·42	0·60	—	0·82
3	72	2·0	0·14	0·22	0·38	0·52	0·70	—	0·72
4*	24	2·0	—	0·15	0·20	0·32	0·45	—	0·52
5†	24	2·0	0·09	0·15	0·27	0·47	0·91	—	1·5
6	24	2·0	—	0·17	0·28	0·46	—	0·75	—
		1·0	—	0·14	0·27	0·50	—	0·74	—
		0·5	—	0·075	0·16	0·33	—	0·84	—

* Incubated in anaerobic jars under hydrogen.
† Using phenobarbitone recrystallized three times from water.

By comparing the amount of growth produced by different quantities of vitamin B_{12} in the presence and in the absence of phenobarbitone, the apparent increase in the concentration of vitamin B_{12} due to the presence of phenobarbitone could be assessed. The results, summarized in Table 1, show that phenobarbitone can cause nearly a sixfold increase in the apparent concentration of vitamin B_{12}. The effect is most marked at the lowest level of vitamin B_{12}, and it is as great after incubation for 72 hr. as after 24 hr. (exp. 3). Anaerobic incubation (exp. 4), and the use of recrystallized phenobarbitone (exp. 5) did not affect the results. Lower concentrations of phenobarbitone caused slightly less marked effects (exp. 6).

Higher concentrations of vitamin B_{12}, between 1 and 5 mμgm. in 5 ml. medium, produced almost the same amount of growth whether phenobarbitone was present or not. In several experiments the amount of growth at these levels of vitamin B_{12} was higher in the absence of phenobarbitone than in its presence.

To test the effect of phenobarbitone on growth in the presence of varying amounts of other growth factors, excess vitamin B_{12} (5 mμgm. in 5 ml. medium) was supplied, and either pteroyl glutamic acid or calcium pantothenate was omitted from the basal medium. With increasing amounts of pteroyl glutamic acid there was a slight reduction in growth in the presence of phenobarbitone over the whole range of concentration. With pantothenate there was a suggestion of stimulation of growth by phenobarbitone at the lower levels, but the effect was much less marked than with vitamin B_{12}.

If phenobarbitone was replaced by thymine or uracil at the same molar concentration, there was little change in the growth response to vitamin B_{12}. Some other barbituric acid derivatives have been tested, also at the same molar concentration as phenobarbitone. Barbituric acid itself had a depressant effect, diethyl barbituric acid had some stimulatory effect though less noticeably than phenobarbitone, while nembutal, amytal and butobarbitone appeared to have stimulatory effects similar to that caused by phenobarbitone.

The property of enhancing growth appears to be connected specifically with vitamin B_{12}, and to be confined to certain barbituric acid derivatives with relatively large substituents in the 5-position. No explanation of the phenomenon has been found, but in view of the chemical resemblance between the barbiturates and the iminazole part of the 5,6-dimethyl benziminazole in the molecule of vitamin B_{12}, it seems possible that barbiturate radicals might replace the benziminazole in vitamin B_{12}, forming analogues of vitamin B_{12} with greater growth-stimulating activities for *Lactobacillus leichmannii* than vitamin B_{12} itself. Experiments are in progress to test this possibility.

I wish to thank Prof. D. D. Woods, of the Microbiology Unit, Department of Biochemistry, University Museum, Oxford, for advice in connexion with these experiments. Thanks are also expressed to Prof. L. J. Witts for his interest in the work, and to Mrs. Elisabeth Ross for assistance with the microbiological assays.

G. H. Spray

Nuffield Department of Clinical Medicine,
Radcliffe Infirmary,
Oxford.
Jan. 2.

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Resistance to Dieldrin in *Lucilia cuprina* Wied., the Australian Sheep Blowfly

While the modified Mules operation[1] greatly assists in reducing the incidence of fly strike of sheep in Australia, chemicals also have an important role in sheep blowfly prevention and control. In agreement with other sheep-raising countries, dieldrin and aldrin have been widely used here for the past three years for the prevention of attack by sheep blowflies.

These insecticides, when used at concentrations of 0·025–0·1 per cent, give periods of protection from 6 to 10 weeks and 12 to 16 weeks against breech and body strike respectively. Aldrin is frequently applied to sheep at twice the concentration of dieldrin.

While dieldrin and aldrin are still affording satisfactory protection against fly strike in sheep throughout New South Wales, there have been recent isolated instances of strikes occurring on sheep within 7–14 days of treatment with either of these insecticides. Field inquiries revealed that the chemicals had been correctly applied, and indicated that insecticidal resistance may have been acquired by *Lucilia cuprina*, which initiates fly strike in sheep in many areas in Australia.

Small numbers of adult flies recently hatched from larvæ, obtained from active strikes on sheep on three properties, currently experiencing difficulty in preventing fly strike with either aldrin or dieldrin.

Comparative toxicity tests have been conducted on flies from the three field strains and from a normal laboratory colony. In this work dieldrin was dissolved in kerosene containing 10 per cent acetone. A volume of 0·25 ml. of insecticidal liquid was applied to the thorax of female flies, seven to ten days old, by means of a microburette[2] while the insects were immobilized with carbon dioxide. The flies were treated with a range of concentrations of 0·01–1·0 per cent dieldrin. The flies were placed, following treatment, in glass containers standing on blotting paper and covered with Petri dish lids. Water and sugar were provided, and mortality records were taken 24 hr. after treatment.

The *LD*50 for the normal laboratory and one field strain of flies is approximately 0·025 μgm. of dieldrin per female fly. Two field strains of flies have an *LD*50 between 1·25 and 2·5 μgm. of dieldrin per female fly. The *LD*50 dosages for the two resistant field strains may be in excess of the maximum figure shown, for the work was undertaken during a heat

wave and there was no provision for lowering the temperature in the chamber in which the treated flies were held. Dieldrin has been shown to have a positive temperature coefficient with *Lucilia cuprina* in earlier investigations.

In addition to conducting tests with adult flies, tests were also made with prepupæ of field and normal strains. Prepupæ were immersed for 5 min. in emulsions of dieldrin and placed in sand following drainage. A complete hatch of flies was obtained from prepupæ of the field strains after immersion in 0·5 per cent dieldrin, but no flies emerged from prepupæ from the normal strain following treatment with 0·05 per cent dieldrin. The prepupæ from the field strains are at least ten times more resistant to dieldrin than those from the normal strain.

The demonstration of resistance to dieldrin in the two field strains of *Lucilia cuprina* reported here is interesting, for a very small increase in resistance has been obtained in a laboratory programme aimed at selecting for resistance to this insecticide. Laboratory selections have been made for twelve generations.

The resistance to dieldrin in the field strains could, however, explain the unsatisfactory results being obtained on the two properties concerned. In view of the rapid development of resistance of dipterous insects to dieldrin[3], it is highly possible that dieldrin resistance in *Lucilia cuprina* will appear in other areas. The laboratory investigations have been confined to studies on dieldrin owing to the small numbers of flies available for the tests. The work will be extended to include toxicity studies with other insecticides used for sheep blowfly prevention.

G. J. Shanahan

N.S.W. Department of Agriculture,
Sydney.
Dec. 31.

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Tryptophan Content of Wool

Previous results for the tryptophan content of wool have given results varying from 0·5 to 2·3 gm. per 100 gm. of wool[1-7]. In the majority of cases the tryptophan has been determined after alkaline hydrolysis of the wool. Spies and Chambers[8] have shown that the determination of tryptophan in casein, after alkaline hydrolysis, is unreliable unless special precautions, such as the use of an atmosphere of hydrogen, are taken. They found that there is reaction of tryptophan with serine, threonine and cystine during hydrolysis, and, since wool contains larger amounts of these amino-acids than casein, the results are likely to be less reliable than those for casein. They developed a method for the estimation of tryptophan in intact proteins by reaction with *p*-dimethylamino-benzaldehyde in 19 *N* sulphuric acid which avoided these side reactions.

We have used a modification of this method to determine the tryptophan content of wool and have obtained very consistent results. 30 mgm. wool, of known moisture content, is added to a solution of 100 mgm. *p*-dimethylaminobenzaldehyde in 18·1 *N* sulphuric acid and the mixture allowed to stand in the dark at room temperature with occasional shaking, until the wool dissolves. This takes about one week. The blue colour is then developed by the addition of 0·1 ml. 0·01 *M* sodium nitrite solution and estimated with a Spekker photoelectric absorptiometer using Chance *OR2* filters.

Table 1. Tryptophan Content of Wool

Wool	Fibre diam. (μ)	gm. Tryptophan per 100 gm. dry wool
Durban merino	16·0	1·07
Port Elizabeth merino	19·8	1·01
64's B.A.	21·3	0·97
56's New Zealand	23·9	0·95
56's Monterides	30·2	0·94
44's New Zealand	34·3	0·93

The tryptophan contents of a number of wool samples determined by this method are given in Table 1.

These results show that there is a tendency for the finer quality wools to contain more tryptophan than those of lower quality. This does not appear to be due to destruction by the sulphuric acid of some of the tryptophan in the coarser wools, since we have obtained the same tryptophan content for a sample of wool using sulphuric acid varying in strength from 17·1 *N* to 23·0 *N* and also using hydrochloric acid in place of the sulphuric acid.

The tryptophan content of a number of other keratins has been determined by this method and the results are given in Table 2.

Table 2. Tryptophan Content of Various Keratins

Keratin	gm. Tryptophan per 100 gm. dry keratin	Previous values
Wool (root ends)	0·93–1·07	0·5 (ref. 1), 0·7 (ref. 2), 0·94 (ref. 3), 1·45 (ref. 4), 1·8 (ref. 5), 2·1 (ref. 6), 2·3 (ref. 7)
Mohair	0·74	—
Camel hair	0·84	0·8 (ref. 2)
Cow hair	0·82	1·4 (ref. 2)
Human hair	0·54	0·7 (ref. 2), 1·0 (ref. 9)
Cashmere	0·97	—
Cow horn	0·84	0·7 (ref. 2), 1·43 (ref. 4)
Finger nails	0·66	1·1 (ref. 2)
Pigeon quill	1·08	—

These results show that the tryptophan contents of the keratin samples examined lie between 0·5 and 1·1 per cent. This contrasts with the occurrence of only trace amounts of tryptophan in various collagens[10,11] and suggests that determination of the tryptophan content of tissue proteins may be a convenient method for classifying the proteins as collagens or keratins.

We hope to publish a fuller account of this work in due course.

We wish to thank the directors of Wolsey, Ltd., for permission to publish this communication.

D. R. Graham
K. W. Statham

Abbey Meadow Mills,
Dyers and Finishers,
Leicester.
Jan. 7.

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Polymely in *Xenopus laevis*

In several genera of the order Anura supernumerary limbs occur naturally[1]. They are usually extra hind limbs[2]. Polymely does not appear to have been reported in the genus *Xenopus*. The female toad (*Xenopus laevis*) described here came with a consignment of toads imported from South Africa in 1956. The toad measured 8·0 cm. from snout to vent and weighed 60 gm. and, except for a supernumerary right arm, was quite normal in appearance. The additional forearm was a mirror image of the normal right arm and was the same size as the two normal limbs. It measured 3·2 cm. from the axilla to the tip of the longest digit. The additional limb was attached immediately anterior to, and in the same plane as, the normal right arm. The point of attachment of the two limbs formed a prominent bony lump (Fig. 1). The extra limb was functionally normal and capable of independent movement. *Xenopus* is aglossate and pushes food into its mouth with both hands. In this specimen the back of the supernumerary hand assisted the normal left hand to carry food to the mouth, and the normal left hand alone was used for pushing food into the mouth. This toad had no difficulty in competing with normal *Xenopus* for food and was well nourished.

The toad had been in the laboratory for more than a year before its abnormality was noticed, and it was only discovered then because the toad was handled.

Fig. 1. Female *Xenopus laevis* with a supernumerary right arm

Fig. 2. The toad in the 'two-armed' position; supernumerary arm concealed

Fig. 2 shows the animal in the 'two-armed' position; the supernumerary limb is concealed by the head. When the toad rested in this position it was indistinguishable from normal animals in the same tank. Accounts of supernumerary limbs in the Anura show that the extra limb is abnormal, being incomplete osteologically and deficient in muscle. This was not the case for the toad described here. The extra limb was functional, and had the same dimensions as the two normal fore limbs. This example of polymelia in the genus *Xenopus* appears to be the first that has been reported, and supernumerary limbs have never been described in the related genera *Pipa*, *Hymenochirus* and *Pseudohymenochirus*. During 1947–57, we have imported 23,250 toads from South Africa, and in the same period 6,480 *Xenopus* have been bred in this Laboratory. No other toad has had a supernumerary limb.

B. M. Hobson

Pregnancy Diagnosis Laboratory
(University of Edinburgh),
Warrender Park Road,
Edinburgh 9. Dec. 18.

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A New Type of Light Organ in the Deep-Sea Fish *Opisthoproctus*

In the closely related salmonoid genera *Opisthoproctus* and *Monacoa* the ventral surface is formed as a flat sole reaching from under the head to the terminal or subterminal anus. Trewavas[1] found an organ immediately under the skin of the sole which she regarded as a swim-bladder. The question why these fishes, in contrast to all other known bathypelagic fish, should have a flat ventral surface and presumably swim upside down called for an explanation.

Dissections and serial sections of specimens from the *Dana* Oceanographical Collections revealed the following facts:

1. *Opisthoproctus* has a swim-bladder in the normal position (Fig. 1, *SW*).

2. The organ in the ventral sole (Fig. 1, *REFL*) is not a swim-bladder. Its lumen is completely filled with a very transparent gelatinous tissue and no *retia mirabilia* or gas gland is present.

3. As described by Trewavas, the so-called swim-bladder is posteriorly attached to a swelling of the rectum (Fig. 1, *RE*). The wall of this rectal bulb is strongly pigmented and contains numerous blood vessels. A sagittal section through the bulb shows that its posterior wall is covered inside by a thick luminous epithelium of similar appearance to that found in the light-organs of ceratioids. Like these, this light-organ is an open gland most probably containing luminous bacteria. In front of the

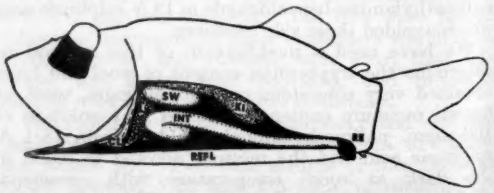

Fig. 1. Sketch of *Opisthoproctus* showing position of swim-bladder (*SW*), rectal light-organ (*RE*) and its reflector organ (*REFL*). Paired fins and gonad removed. (*CL*, cleithrum; *KI*, kidney; *L*, liver)

luminous epithelium is a transparent lens-like structure consisting of two lateral halves filling the rest of the bulb, leaving only a narrow slit for the passage of the fæces.

4. The wall of the rectal bulb, which elsewhere is thick and pigmented, has a very thin and completely transparent window where it is attached to the bladder-like organ of the ventral sole. Dorsally the inner surface of this 'bladder' has a silky brilliance.

Evidently the function of the organ in the sole of *Opisthoproctus* is to serve as a reflector for the rectal light-organ. The light is transmitted through the lens and window of the bulb to the crystal-clear tissue of the reflector organ. From the shining ventral wall of the latter the light is reflected downwards and dispersed over the whole ventral surface of the fish. The flat sole is covered with large thin scales which are transparent at the base but pigmented with increasing density towards the distal edge. A shortening of the sole will thus screen the light.

Probably the main biological significance of this complicated organ is that it enables the fish to be recognized by others of its kind which have sensitive, upwardly directed, tubular eyes. Possibly a very low light intensity is sufficient for this purpose. This would greatly reduce the risk of being detected by predators, and furthermore the downwardly directed light from the flat sole will be difficult to observe by fish with normal horizontally directed eyes.

Somewhat similar types of light-organs with open glands connected more or less directly with the intestine, and emitting light indirectly through transparent parts of the body, have been found in some Percomorphi from shallow water: *Acropomona japonicum*, *Leiognathus equula*, *Gazza minuta* and *Apogon marginatus*[2-5].

A more detailed report on the rectal light-organ of *Opisthoproctus* and related genera is under preparation and will appear in the "*Dana* Reports".

E. Bertelsen

Dana Oceanographical Collections,
Charlottenlund.
Jan. 6.

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Effect on Earthworm and Diptera Populations of BHC Dust applied to Soil

It is well known that BHC affects earthworms and soil insects. A review of these problems has been given by Satchell[1].

In May 1956, 12 per cent γ-BHC dust was applied at surface concentrations of 10·08, 20·16, 28·8 and 57·6 gm./100 m.² to ploughed plots and was thoroughly mixed with soil to a depth of 4 in. In 1956 and 1957 the plots were sown with sugar beet and potato. In October 1957 (18 months after treatment with BHC) the earthworms and other fauna were extracted from the experimental plots.

A significant increase in the earthworm population occurred in all the plots treated with BHC (Table 1). This may be partly explained by 'food mobilization' in soil treated with BHC[2].

A statistical examination has shown that there is a curvilinear correlation (parabolic) between BHC concentration and the number of earthworms. The optimal γ-BHC concentration is 31·03 gm./100 m.². This gives the highest number of earthworms.

Table 1. Effect of BHC on Numbers of Earthworms in Potato Plots

gm./100 m.² of γ-BHC	No. of earthworms per m.²
Untreated	10·0
10·08	21·6
20·16	25·8
28·8	26·1
57·6	14·6

Table 2. Effect of BHC on Populations of Wireworm and Diptera in Sugar Beet Plots

gm./100 m.² of γ-BHC	No. per m.²	
	Wireworm	Diptera
Untreated	1·1	3·6
10·08	0·2	4·3
20·16	0·2	6·0
28·8	—	7·1
57·6	—	6·6

It was observed (Table 2) that on all treated sugar beet plots the number of wireworm was reduced and the population of Diptera (*Pegomyia hyoscyami* and others) was increased. It is suggested that the significant increase in the number of Diptera in all plots treated with BHC was due to a reduction in predators, the numbers of wireworm. Similar results were obtained by Stone[3] with the wireworms *Limonius californicus* and *Hylemyia ciliclura*.

Jerzy J. Lipa

Laboratory of Agricultural Entomology,
Institute of Plant Protection,
Puławy, Poland.
Dec. 18.

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Thermocline off the Coast of Tropical West Africa

Recently, Salzen[1] has shown that there is a marked difference with depth in the species composition of the demersal fish fauna off Tema in Ghana. He found that there was a distinct change from euryhaline and eurythermal species in the shallow inshore water, to red and brown marine species below 10 fathoms. As the same change in species had been observed by Postel[2] working off French Guinea, Salzen suggested that the results for Tema might be typical of a great deal of the West African shelf in the dry season. The results (unpublished) of an extensive, though not detailed, hydrographic survey of the region, from Dakar to the Cameroons, made during 1952, tend to support such a generalization, and pose the question as to whether the thermocline might be partly responsible for determining the distribution of the inshore fish fauna.

During the course of the hydrographic survey, a number of bathythermograph recordings were made at stations distributed in transects at various intervals along the West African shelf. Salinity samples were also taken using a Nansen Petersen insulated water-bottle. The results of the survey were in close agreement with the observations of Defant[3] made during the *Meteor* Expedition of 1925–27. It was found that the deeper water on the shelf was separated

from the surface waters by a layer within which the density increased so rapidly with depth that it had the character of a discontinuity layer and within this layer there was a distinct salinity maximum. The discontinuity layer or thermocline covered the whole area of the survey and showed a slight slope to seawards.

The water masses above the thermocline showed a small and irregular vertical temperature gradient, but within the thermocline itself, the gradient was very marked—about 9 deg. C. in 13 fathoms off the coast of Sierra Leone, and about 4 deg. C. in 5 fathoms off Ghana. The salinity gradient was most pronounced in the surface waters, especially near the coast. On proceeding away from the coast, the salinity of the surface waters increased rapidly, and at the same time the temperature decreased. Off the coast of Ghana, however, this tendency was reversed, presumably as a result of upwelling near the coast. In this region, colder and more saline water was found close to the surface inshore, and as a result the thermocline had a smaller vertical extension than elsewhere in the area of the survey.

Table 1. Average Depth of the Thermocline off the Coast of the British West African Territories

Coast	No. of stations	Average depth from surface	
		Bottom of surface water (fathoms)	Bottom of thermocline (fathoms)
Gambia	12	10	24
Sierra Leone	10	12	28
Ghana	14	10	15
Nigeria	16	15	26

The average depth of the thermocline is indicated in Table 1. It will be seen that the bottom of the surface water approximates to that depth (10 fathoms) at which Postel and Salzen observed the change from euryhaline and eurythermal species to predominately marine. This suggests that the thermocline probably plays some part in determining the nature and distribution of the inshore fish fauna of West Africa. Unfortunately, the results of the 1952 hydrographic survey are insufficient to allow any precise relationships to be determined, but in view of the fact that the thermocline is a constant and widespread feature of the waters on the West African shelf, its presence should, perhaps, be considered as a contributory cause of the correspondence between the Tema area and the French Guinea shelf.

J. C. D. Watts

Fisheries Development and Research Unit,
Freetown.
Dec. 31.

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Bixbyite from Nagpur

During an examination of polished sections from Prof. A. M. Bateman's collection of Indian manganese made during his trip in 1954 (1232*a*, Joda East Nagpur), I noticed a few grains of a greyish white mineral with a distinct yellow tint and a slightly greater hardness than braunite, with which it is associated. Etch tests with nitric acid, potassium cyanide, stannous chloride and hydrogen peroxide were negative. Microchemical tests indicated the presence of manganese and iron. X-ray photographs of the mineral were taken by the Debye–Scherrer method using iron *K* radiation. The *d* spacings observed from the film, as given in Table 1, agree fairly well with the values obtained by Fleischer and Richmond[1] for bixbyite from the Thomas Range in Utah.

Table 1

d	*I*	*d*	*I*	*d*	*I*	*d*	*I*
4·52	1	2·14	6	1·49	1	1·10	2
3·34	1	1·87	1	1·46	2	1·14	1
3·19	2	1·80	2	1·40	8	1·07	8
2·98	1	1·71	1	1·35	2	1·05	7
2·73	10	1·66	9	1·25	1	1·03	2
2·33	4	1·53	2	1·17	1		

d, Spacings in Angstrom units; *I*, estimated intensities

Fleischer and Richmond give the formula $(Mn/Fe)_2O_3$ for bixbyite. Mason[2] has shown the chemical similarity and structural identity of bixbyite and sitaparite.

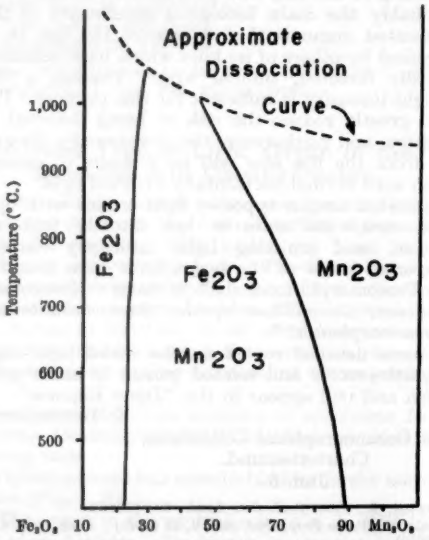

Fig. 1. The system Fe_2O_3—Mn_2O_3 (after Mason, *Amer. Mineral.*, **29**; 1944)

Fig. 1 indicates that the solubility of Fe_2O_3 in Mn_2O_3 increases with increasing temperature.

Bixbyite formed at high temperatures contains 45–60 per cent Fe_2O_3. Sitaparite formed during metamorphism of manganese ores contains about 30 per cent Fe_2O_3. Unmixing of these minerals into iron-poor sitaparite and iron-rich bixbyite has significance in geological thermometry.

Bixbyite has been reported from many localities including Patagonia, Sweden, South Africa, Utah and New Mexico. A survey of the literature shows that this is the first report of the mineral from India.

The above observations were made during a study of Indian manganese ores at the Laboratory of Economic Geology, Yale University.

I am grateful to Prof. Alan M. Bateman for his direction and valuable help.

B. L. Sreenivas

Laboratory of Economic Geology,
Yale University,
New Haven, Conn.

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FORTHCOMING EVENTS

(Meeting marked with an asterisk • is open to the public)

Monday, March 24

ROYAL GEOGRAPHICAL SOCIETY (at 1 Kensington Gore, London, S.W.7), at 5 p.m.—Prof. W. G. V. Balchin: "A Water Use Survey".

Monday, March 24—Thursday, March 27

PHYSICAL SOCIETY (in the Old and New Halls of the Royal Horticultural Society, off Victoria Street, London, S.W.1)—Annual Exhibition of Scientific Instruments and Apparatus.

Tuesday, March 25

INSTITUTION OF CIVIL ENGINEERS (at Great George Street, London, S.W.1), at 5.30 p.m.—Mr. Gerald Lacey: "Flow in Alluvial Channels with Sandy Mobile Beds".

INSTITUTION OF ELECTRICAL ENGINEERS, MEASUREMENT AND CONTROL SECTION (at Savoy Place, London, W.C.2), at 5.30 p.m.—Dr. L. Essen: "The Atomic Clock".

SOCIETY OF CHEMICAL INDUSTRY, PLASTICS AND POLYMER GROUP (at 14 Belgrave Square, London, S.W.1), at 6.30 p.m.—Mr. D. K. Hale: "Ion Exchange Resins".

ROYAL PHOTOGRAPHIC SOCIETY (at 16 Princes Gate, London, S.W.7), at 7 p.m.—Annual General Meeting.

Tuesday, March 25—Thursday, March 27

INSTITUTION OF NAVAL ARCHITECTS (in the Weir Lecture Hall, 10 Upper Belgrave Street, London, S.W.1), at 10.15 a.m. daily—Spring Meeting.

SOCIETY OF CHEMICAL INDUSTRY (at the University, Aberdeen)—Conference on "Fundamental Aspects of the Dehydration of Foodstuffs".

Wednesday, March 26

GEOLOGICAL SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—Mr. N. L. Falcon and Dr. P. E. Kent: "The Geological Results of Petroleum Exploration in Britain, 1945 to 1957".

INSTITUTE OF FUEL (at the Institution of Civil Engineers, Great George Street, London, S.W.1), at 5.30 p.m.—Dr. V. R. Gray: "The Dewatering of Fine Coal".•

INSTITUTION OF ELECTRICAL ENGINEERS, SUPPLY SECTION (at Savoy Place, London, W.C.2), at 5.30 p.m.—Prof. F. Giordani: "The Objective for Euratom".

BRITISH INSTITUTION OF RADIO ENGINEERS (at the London School of Hygiene and Tropical Medicine, Keppel Street, Gower Street, London, W.C.1), at 6.30 p.m.—Mr. W. J. Perkins and Dr. M. H. Evans: "Electronics in Medical Research".

SOCIETY FOR ANALYTICAL CHEMISTRY, BIOLOGICAL METHODS GROUP, (in the Physiology Laboratory, University College, Gower Street, London, W.C.1), at 6.30 p.m.—Demonstration Meeting.

ROYAL INSTITUTE OF CHEMISTRY (joint meeting with the CHELSEA COLLEGE OF SCIENCE AND TECHNOLOGY CHEMICAL SOCIETY, at the Chelsea College of Science and Technology, Manresa Road, London, S.W.3), at 7 p.m.—Dr. F. L. Rose, F.R.S.: "Chemotherapy".

Thursday, March 27

PHOTOBIOLOGY GROUP (at Bedford College, Regent's Park, London, N.W.1), at 2 p.m.—Dr. C. A. Parker: "Recent Advances in Spectrofluorimetry and Chemical Actinometry"; Dr. R. S. Spencer: "The Biology of Luminous Bacteria"; Mr. E. S. Trickett: "Problems of Radiation Measurement in Relation to Environmental Control".

BONE AND TOOTH SOCIETY (in the New Lecture Theatre, The Hospital for Sick Children, Great Ormond Street, London, W.C.1), at 5 p.m.—Symposium on "Eosinophilic Granuloma and Related Conditions". Speakers: Dr. Reginald Lightwood, Dr. John Sutcliffe and Dr. Martin Bodian.

OIL AND COLOUR CHEMISTS' ASSOCIATION (at the Royal Society of Tropical Medicine and Hygiene, Manson House, 26 Portland Place, London, W.1), at 7 p.m.—Mr. C. C. Mill: "Some Observations on the Rheology of Carbon Black Suspensions".

Thursday, March 27—Friday, March 28

INSTITUTION OF ELECTRICAL ENGINEERS, RADIO AND TELECOMMUNICATION SECTION (at Savoy Place, London, W.C.2)—Convention on "Radio Aids to Aeronautical and Marine Navigation".

Friday, March 28

BIOCHEMICAL SOCIETY (at the University of Birmingham Medical School), at 11 a.m.—Scientific Papers.

BRITISH PSYCHOLOGICAL SOCIETY, INDUSTRIAL SECTION (in the Department of Psychology, Birkbeck College, Malet Street, London, W.C.1), at 1 p.m.—Mr. J. N. Bamforth: "Human Relations Training at Roffey Park—an Experiment in Evaluation of Results".

ROYAL ASTRONOMICAL SOCIETY (at Burlington House, Piccadilly, London, W.1), at 4.15 p.m.—Geophysical Discussion on papers received during the Session 1957–58. Chairman: Dr. R. Stoneley, F.R.S. Speakers: Dr. A. H. Cook, Mr. H. M. Iyer, Sir Harold Jeffreys, F.R.S., and Mr. A. S. Merriweather.

MEDICAL RESEARCH COUNCIL (at the National Institute for Medical Research, Mill Hill, London, N.W.7), at 5 p.m.—Prof. N. I. Kalabukhov (Institute for Scientific Research, Ministry of Health, Saratov, U.S.S.R.): "Seasonal Changes in the Organism of Mammals in Relation to the Environment".

INSTITUTION OF MECHANICAL ENGINEERS, INTERNAL COMBUSTION ENGINE GROUP (at 1 Birdcage Walk, Westminster, London, S.W.1), at 6 p.m.—Discussion on "The Ultimate Engine: Stationery, Rail Traction and Marine".

ROYAL PHOTOGRAPHIC SOCIETY, KINEMATOGRAPH GROUP (at 16 Princes Gate, London, S.W.7), at 7 p.m.—Annual General Meeting, followed by a review of recent apparatus, by Mr. A. R. Rigg.

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 9 p.m.—Prof. Richard Julius Pumphrey, F.R.S.: "Hearing in Man and Animals".

Friday, March 28—Saturday, March 29

LINNEAN SOCIETY OF LONDON (in the Lecture Theatre of the British Museum (Natural History), Cromwell Road, London, S.W.7), at 10.45 a.m. Friday and 10.15 a.m. Saturday—Symposium on "The Uses of the Electron Microscope in Biology".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

ASSISTANT (SCIENTIFIC) IN THE DEPARTMENT OF PHYSIOLOGY AND PLANT CULTURE, for routine duties connected with plant studies in controlled environments—The Secretary, John Innes Horticultural Institution, Bayfordbury, Hertford, Herts (March 27).

SCIENTIFIC OFFICER (with an honours degree in plant physiology) IN THE IRRIGATION SECTION—The Secretary, National Vegetable Research Station, Wellesbourne, Warwick (March 28).

ASSISTANT PHYSICIST, Basic grade (with an honours degree in physics), for duties mainly in the X-Ray Diagnostic Department—The Personnel Officer, St. Thomas's Hospital, London, S.E.1 (March 29).

HOSPITAL BIOCHEMIST, Senior grade (with a university degree in science or equivalent qualification), in the routine Biochemical Section of the Department of Clinical Biochemistry—The Administrator, Radcliffe Infirmary, Oxford (March 29).

DEMONSTRATOR IN PHYSICS—The Registrar, The University, Hull (March 31).

NUFFIELD SCIENCE TEACHING FELLOW (with a good honours degree in physics and preferably some experience in research or advanced study) IN THE PHYSICS DEPARTMENT of the Faculty of Science, to pursue research in physics and at the same time participate in teaching at local grammar schools—Prof. Samuel Devons, Physical Laboratories, The University, Manchester 13 (March 31).

JUNIOR (ASSISTANT) LECTURER IN BIOCHEMISTRY, with special reference to physical biochemistry—The Dean, Guy's Hospital Medical School, London, S.E.1 (April 1).

LECTURER (honours graduate) IN PHYSICS at Kumasi College of Technology—The Council for Overseas Colleges, 12 Lincoln's Inn Fields, London, W.C.2 (April 4).

IMPERIAL CHEMICAL INDUSTRIES RESEARCH FELLOWS IN CHEMISTRY, PHYSICS, ENGINEERING (including CHEMICAL ENGINEERING), METALLURGY, PHARMACOLOGY or related subjects—The Registrar, The University, Sheffield (April 5).

READER IN SOIL SCIENCE—The Secretary, The University, Aberdeen (April 7).

LECTURER (preferably with experience in reactor physics or nuclear metallurgy) IN THE NUCLEAR ENGINEERING LABORATORY—The Registrar, Queen Mary College (University of London), Mile End Road, London, E.1 (April 8).

SENIOR TUTOR (PHYSICIST) (preferably with university teaching experience) IN THE DEPARTMENT OF PHYSICS—The Registrar, University College of Swansea, Singleton Park, Swansea (April 8).

LECTURER IN CIVIL OR MECHANICAL ENGINEERING—The Registrar, University College, Singleton Park, Swansea (April 10).

RESEARCH FELLOW (with a Ph.D. degree or equivalent experience) IN THE CHEMISTRY DEPARTMENT, for work on the kinetics of gaseous oxidation—The Professor of Chemistry, The University, Hull (April 10).

ASSISTANT LECTURER IN GEOGRAPHY, and an ASSISTANT LECTURER IN PHILOSOPHY—The Registrar, The University, Leicester (April 12).

TURNER AND NEWELL RESEARCH FELLOW IN ENGINEERING, INORGANIC CHEMISTRY or PHYSICS or other allied science—The Registrar, The University, Leeds 2 (April 12).

LECTURER (with special qualifications in physical chemistry) IN THE DEPARTMENT OF CHEMISTRY—The Registrar, University College, Singleton Park, Swansea (April 14).

SCIENTIFIC OFFICER (with qualifications which include an honours degree in natural sciences), for research on resistance of sugar beet to beet eelworm—The Secretary, The School of Agriculture, The University, Downing Street, Cambridge (April 15).

SENIOR LECTURER (with a knowledge of tropical pastures and of animal nutrition in humid tropics) IN AGRICULTURE (NUTRITION) at University College, Ibadan, Nigeria—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1 (April 20).

LECTURER (preferably with experience in electronics and instrumentation for automatic control) IN ELECTRICAL ENGINEERING—The Registrar, Queen Mary College (University of London), Mile End Road, London, E.1 (April 30).

LECTURER or ASSISTANT LECTURER IN ANATOMY, and a LECTURER or ASSISTANT LECTURER IN BACTERIOLOGY at the University of Malaya, Singapore—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1 (April 30).

READER IN BIOCHEMISTRY at the University of Adelaide, Australia—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (Australia, April 30).

SENIOR LEVERHULME RESEARCH FELLOW, for research in some aspects of radio-astronomy, using the radio-telescope at the Jodrell Bank Experimental Station—The Registrar, The University, Manchester 13 (April 30).

LECTURER or ASSISTANT LECTURER (with interests in physical and/or inorganic chemistry) IN CHEMISTRY at the University College of the West Indies—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1 (May 3).

LEVERHULME STUDENTS (graduates in chemistry or engineering) IN CHEMICAL ENGINEERING, for advanced training or research in chemical engineering in the Ramsay Memorial Laboratory—The Registrar, University College, Gower Street, London, W.C.1 (May 15).

LECTURER IN PHYSICS at the University College of Rhodesia and Nyasaland—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1 (May 20).

APPLIED MATHEMATICIAN, to provide mathematical services in forestry, fisheries and wild-life management research; a FOREST FIRE PHYSICIST (preferably with a background in meteorology), to initiate a field research programme on physical and meteorological factors influencing growth and spread of forest fires; and an OCEANOGRAPHER (physicist, preferably, with a background of hydrodynamics, meteorology or oceanography, and competent in mathematics), to lead scientific parties aboard the research vessel, taking oceanographic and meteorological observations, developing new observational techniques and instruments, analysing data and undertaking model work—Head of Physics Section, Division of Research, Department of Lands and Forests, R.R. No. 2, Maple, Ontario, Canada.

ASSISTANT PROFESSOR IN THE DEPARTMENT OF PHYSICS—The University of New Brunswick, Fredericton, New Brunswick, Canada.

ASSISTANT (with Inter.B.Sc. or equivalent qualifications including mathematics) IN THE COMPUTING MACHINE LABORATORY, for miscellaneous duties in connexion with the preparation of work for an electronic digital computer—Dr. T. Kilburn, The Department of Electrical Engineering, The University, Oxford Road, Manchester 13.

HEAD (mechanical engineer, with good teaching, industrial and/or research experience) OF THE DEPARTMENT OF ENGINEERING—The Principal, Welsh College of Advanced Technology, Cathay's Park, Cardiff.

LECTURER IN MATHEMATICS, to teach pure and applied mathematics to degree standard—The Clerk to the Governors, Northern Polytechnic, Holloway, London, N.7.

LECTURER (with a suitable degree and industrial or research experience) IN METALLURGY—The Deputy Registrar, The University, Birmingham 15.

MASS SPECTROSCOPIST (with a first- or second-class honours degree and preferably research experience in mass spectroscopy and a knowledge of electronics and vacuum technology), to work on the development of a high-resolution mass spectrometer—Group Recruitment Officer (1045Y/34), Atomic Energy Research Establishment, Harwell, Berks.

PHYSICIST, Experimental Officer or Assistant Experimental Officer grade (with at least G.C.E. in five subjects, with physics and one other scientific or mathematical subject at advanced level or equivalent), to work in a team engaged on specialized nuclear radiation measurements—The Senior Recruitment Officer, Atomic Weapons Research Establishment, Aldermaston, Berks, quoting Ref. 1733/34.

TECHNICIAN (preferably with a knowledge of photography and/or geological techniques) IN THE GEOLOGY DEPARTMENT—The Registrar (GT), Queen Mary College (University of London), Mile End Road, London, E.1.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Report of the Committee appointed by the Prime Minister to examine the Organisation of certain parts of the United Kingdom Atomic Energy Authority. Pp. 40. (Cmnd. 338.) (London: H.M. Stationery Office, 1957.) 2*s.* net. [3012

Report of the Thirty-Fifth Meeting of the Ross Institute Industrial Advisory Committee—Housing for Estate Labour. Pp. 20. (London: London School of Hygiene and Tropical Medicine incorporating the Ross Institute, University of London, 1957.) [3112

Ministry of Agriculture, Fisheries and Food. Fishery Investigations. Series 1, Vol. 6, No. 1: Hydrographical Survey of the River Usk. By A. Swain and O. F. Newman. Pp. v+34. (London: H.M. Stationery Office, 1957.) 8*s.* 6*d.* net. [3112

London and Home Counties Regional Advisory Council for Higher Technological Education. Bulletin of Special Courses in Higher Technology, including Management Studies and Commerce, 1957–58. Part 2: Spring and Summer Terms, 1958. Pp. 70. (London: London and Home Counties Regional Advisory Council for Higher Technological Education, 1957.) 3*s.* [3112

The Registrar General's Statistical Review of England and Wales for the year 1956. Part 1: Tables, Medical. Pp. x+372. (London: H.M. Stationery Office, 1957.) 15*s.* net. [3112

Aluminium Development Association. Information Bulletin No. 23: Soldering Aluminium. Pp. 35. (London: Aluminium Development Association, 1957.) 2*s.* [3112

Proceedings of the Association of British Chemical Manufacturers Work Study Conference for Senior Work Study Practitioners and Works Managers, Hotel Majestic, Harrogate, 23rd to 25th May, 1957. Pp. v+206. (London: Association of British Chemical Manufacturers, 1957.) 13*s.* 6*d.*, post free. [3112

Other Countries

Rijkslandbouwhogeschool, Gent. Negende Jaarlijks Symposium over Phytopharmacie, 7 Mei 1957. Pp. iv+335–702. (Overdruk uit "Mededelingen van de Landbouwhogeschool en de Opzoekingsstations van de Staat te Gent", 1957, Deel 22, No. 3.) (Gent: Rijkslandbouwhogeschool, 1957.) [3112

Sveriges Geologiska Undersökning. Avhandlingar och Uppsatser. Ser. C, No. 549: The Titaniferous Ore-Bearing Gabbro of Helsingland, Central Sweden. By P. H. Lundegårdh. Pp. 34. (Stockholm: Kungl. Boktryckeriet P.A. Norstedt & Soner, 1957.) 2 kronor. [3112

Indian Council of Medical Research. Technical Report of the Scientific Advisory Board for the year 1956. Pp. xii+290. (New Delhi: Indian Council of Medical Research, 1957.) R.1. [3112

Proceedings of the New Zealand Ecological Society. No. 3: Report of Fourth Annual Meeting, Christchurch, 18th to 20th August, 1955. Pp. 36. 3*s.* No. 4: Report of Fifth Annual Meeting, Wellington, 10th to 12th May, 1956. Pp. 48. 7*s.* 6*d.* (Wellington: New Zealand Ecological Society, 1956 and 1957.) [3112

Research Bulletin of the Panjab University. No. 103, (Zoology): The Millipede Egg. By G. P. Sharma and O. B. Chhotani. Pp. 241–250. Rs. 2–5–0. No. 104, (Zoology): Spermatogenesis of Cicindela, with special reference to Acrosome-Formation, Centrosome and Nuclear Vesicle. By Vishwa Nath, B. L. Gupta and S. Mehta. Pp. 251–259. Rs. 1–80–0. No. 105, (Zoology): Mantodean Oothecae. By G. L. Arora and Indermohan Singh. Pp. 261–267. Rs. 1–0–0. No. 106, (Zoology): On a New Trypanosome from the Blood of an African Gecko, *Hemidactylus Brookii Angulatis* Gray. By Manohar Singh Grewal. Pp. 269–281. Rs. 2–0–0. No. 107, (Mathematics): Partitions Into Exactly *k* Distinct Primes. By O. P. Gupta. Pp. 283–290. Rs. 1–30–0. No. 108, (Zoology): Some Critical Observations on the Feeding Activity of the Desert Locust, *Schistocerca Gregaria* (Forskal), under Different Environmental Conditions. By Asket Singh. Pp. 291–298. Rs. 1–0–8. No. 109, (Mathematics): An Analogue of a Problem of Mahler. By R. P. Bambah. Pp. 299–302. Rs. 1–12–0. No. 110, (Chemistry): Synthesis of Fat by *Penicillium aurantiobrunneum* from Different Sucrose Concentrations at Different Periods of Incubation. By Jagjit Singh and Pushpinder Nath. Pp. 303–307. Rs. 1–0–0. No. 111, (Chemistry): Heat of Wetting of Porous Solids in Relation to their Surface Area. By Balwant Rai Puri, Sukarma Mittal and Lekh Raj Sharma. Pp. 309–315. Rs. 1–16–0. No. 112, (Zoology): Mitochondria and Golgi Bodies in the Spermatogenesis of *Periplaneta americana* as Studied under the Phase-Contrast Microscope. By B. L. Gupta and P. Sehgal. Pp. 317–326. Rs. 1–90–0. No. 113, (Zoology): On Two New Species of the Genus *Neodiplostomum* Railliet, 1919, and Discussion on the Validity of *Neodiplostomum* (*Neodiplostomum*) Rufeni, Chatterji, 1942. By N. K. Gupta. Pp. 327–335. Rs. 1–50–0. No. 114, (Chemistry): Diethylenetetra-Ammonium Sulphatocerate as Volumetric Reagent. 4: Direct Determinations with Indicators. By Balwant Singh. Pp. 337–339. Rs. 0–80–0. No. 115, (Chemistry): Volumetric Studies in Oxidation-Reduction Reactions. Oxidation with Chloramine-T. Determination of Metals. By Balwant Singh and S. K. Soni. Pp. 341–344. Rs. 0–90–0. (Hoshiarpur: Panjab University, 1957.) [3112

Library Science and Scientific Method. By Dr. S. R. Ranganathan. (Reprinted from "The Five Laws of Library Science," Edition 2.) Pp. ii+355–424. (Madras: Madras Library Association, 1957.) [3112

British Guiana. Report on the Geological Survey Department for the year 1956. Pp. 32. (Georgetown: Geological Survey, 1957.) 50 cents; 2*s.* 1*d.* [3112

Chicago Natural History Museum. Fieldiana: Anthropology. Vol. 36, No. 8, (November 8, 1957): The Old Copper Culture and the Keweenaw Waterway. By George I. Quimby and Albert C. Spaulding. Pp. 189–201. (Chicago, Ill.: Chicago Natural History Society, 1957.) 40 cents. [3112

University of Helsinki: Institute of Meteorology. Papers, No. 83: On the Observed Zonal and Meridional Circulations at Latitudes 15° N and 30° N in Winter. By P. A. Vuorela. Pp. 105–120. (Reprint from *Geophysica*, Vol. 6, No. 2.) (Helsinki: University of Helsinki, Institute of Meteorology, 1957.) [3112

Centro Brasileiro de Pesquisas Fisicas. Notas de Fisica. Vol. 2, No. 13: Studies on the Nuclear Photoeffect. By M. D. de Souza, Santos, J. Goldemberg, R. R. Pieroni, E. Silva, Ottavia A. Borrello, Suzanna S. Villaca, and J. Leite Lopes. Pp. 20. Vol. 3, No. 21: On the Capture of Negative Muons by Light Nuclei. By J. Meite Lopes. Pp. 20. Vol. 3, No. 22: Scattering of Nucleons Nuclei in the 30-MeV Region. By L. R. B. Elton and L. C. Gomes. Pp. 18+9 figures. Vol. 3, No. 23: Energy Spectrum of Pions and Polarization of Muons and Electrons in the K 3_π and K$_{e3}$ Decay. By S. W. MacDowell. Pp. 20+5 figures. Vol. 3, No. 24: Note on the Interaction of Elementary Particles. By L. Leite Lopes. Pp. 16. (Rio de Janeiro: Centro Brasileiro de Pesquisas Fisicas, 1957.) [3112

Bulletin of the American Museum of Natural History. Vol. 113, Article 6: Notes and Descriptions of Some Neotropical Muscidae (Diptera). By Fred M. Snyder. (New York: American Museum of Natural History, 1957.) 1 dollar. [3112

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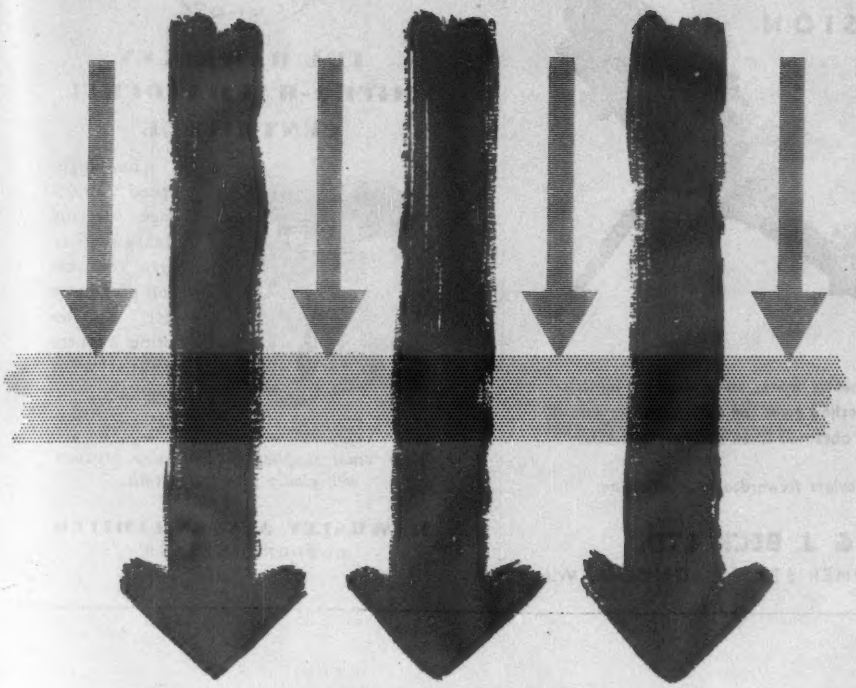

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